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The American Historical Review

AMERICAN HISTORICAL ASSOCIATION

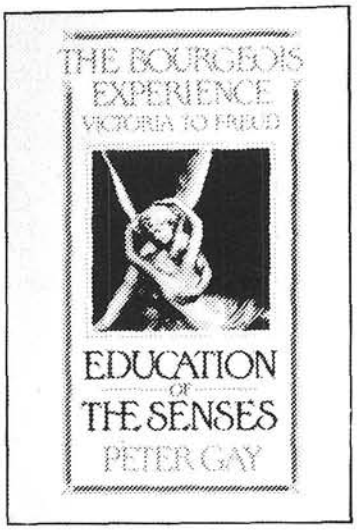
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In This Issue

The first two essays analyze the role of continuous, low-level violence in shaping popular attitudes, which, in the cases examined here, gave added intensity and significance to subsequent upheavals. BARBARA DIEFENDORF explains the fury of the St. Bartholomew's Day massacre as the culmination of a decade-long escalation in anti-Protestant violence. The subject has been explored before, most notably by Natalie Zemon Davis and Janine Estèbe, but Diefendorf's account offers richer detail and rests on a broad base of published and archival sources. Her vivid descriptions of popular attitudes and closely documented analysis of what the authorities knew of them strongly suggest that responsibility for the massacre lay squarely with those in power.

The essay by LOUIS PÉREZ, JR., draws on the ideas of Eric Hobsbawm in linking social disorder in late nineteenth-century Cuba to banditry. He demonstrates the connections among economic depression, the rationalization of sugar production, and the expulsion of small cultivators and shows widespread banditry to have been the most effective response of the displaced or threatened smallholders to the encroachment of rural capitalism. Banditry, in giving form to rural protest, added a powerful social impetus to the political revolution that erupted in 1895 under the banner of Cuba Libre.

The other two contributions illuminate the social history of Central Europe. JEAN QUATAERT presents a major reinterpretation of the forces that shaped women's work in goods manufacture. She begins with a previously unexplored conflict between guilds and the household. Threatened by capitalist advances, artisans lumped together household production and women's work in a definition of gender role designed to exclude household products from the exchange economy. When the government later took over the role of the guilds in protecting the market, Prussian state officials sought to separate wagework and housework by mandating different training sequences, but, like the artisans before them, the officials ran up against the fundamental ambiguity in the two spheres of work. An important consequence was that wagework in garment making, one of the largest employers of female labor in the late nineteenth century, was kept nonprofessional and low skilled because it was associated with negative values that had been imparted to nonguild production.

The height of recruits in the Austrian army provides a proxy measure of nutrition in the Habsburg empire. So argues JOHN KOMLOS in a research note that ties changes in stature to the economic development of the eastern and western halves of the empire and provides new "food" for thought about the impact of industrialization on living standards.

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Prologue to a Massacre: Popular Unrest in Paris, 1557–1572

BARBARA DIEFENDORF

AUGUST 24, 1572—ST. BARTHOLOMEW'S DAY by the Catholic calendar—is an infamous day in history. The slaughter on that day of some two thousand people in the streets of Paris set off tremendous shock waves throughout Europe. The murder of the Huguenot leaders and the defection of thousands of terror-stricken followers thinned the ranks of French Protestantism. Public opinion polarized as Protestants and Catholics alike sought allies for the war that was inevitably to come.[1] The massacre provides a graphic illustration of the savagery of the religious strife, and historians have found in it important clues to the political sentiments of Charles IX, Catherine de Medici, and other primary figures in the conflicts. The question of responsibility for the massacre has been much debated. Accounts of the midnight meeting in the Louvre that resulted in the order to slay the Huguenot leaders have been minutely examined to determine the extent of premeditation and the complicity of the persons who allegedly ordered the crime. Far less attention has been paid to the questions of how and why the action, brutal but limited, touched off a citywide orgy of violence.[2]

An earlier version of this article was presented at the Sixteenth Century Studies Conference held in St. Louis, Missouri, October 1984. I am grateful to Philip Benedict for the comments he made on that paper and to the National Endowment for the Humanities for a fellowship that supported this research.

[1] Estimates of the number of victims of the massacre vary tremendously. I have accepted the relatively conservative figure offered by Janine Estèbe. See Estèbe, *Tocsin pour un massacre: La saison des Saint-Barthélemy* (Paris, 1968), 201. For recent assessments of the political consequences of the massacre, see John H. M. Salmon, *Society in Crisis: France in the Sixteenth Century* (New York, 1975), 187–93; Janine Garrisson-Estèbe, *Protestants du Midi, 1559–1598* (Toulouse, 1980), 177–79; Nicola M. Sutherland, *The Huguenot Struggle for Recognition* (New Haven, Conn., 1980), 209; and Alfred Soman, ed., *The Massacre of St. Bartholomew: Reappraisals and Documents* (The Hague, 1974). Philip Benedict made a strong case for the massacre as a turning point in the religious wars because of the massive defections and general discouragement that followed. See his *Rouen during the Wars of Religion* (Cambridge, 1981), 125–50.

[2] For important exceptions, see Estèbe, *Tocsin pour un massacre*; and Ilja Mieck, "Die Bartholomäusnacht als Forschungsproblem: Kritische Bestandsaufnahme und neue Aspekte," *Historische Zeitschrift*, 216 (1973): 73–110. Estèbe conceded that "St. Bartholomew's Day was not an explosion that came out of nowhere. To borrow a classic metaphor, the powder had already been accumulating for years, and it had been touched off several times already"; *Tocsin pour un massacre*, 75. She did not, however, explore the build-up of tensions in any great detail. Her analysis focuses rather on the massacre as a "class crime," a "pogrom," and a "ritual crime"; *ibid.*, 196–97. Although I would agree that there are elements of ritual crime—a desire to purify the social body—in the massacre, I believe that Estèbe overstated the elements of class crime and pogrom. Pillaging was not a primary motive, and the Huguenots were not yet considered a separate "race" or people. My differences with Mieck's interpretation are more serious. I find no evidence for the existence of a band of irregulars or auxiliary troops under the direction of Claude Marcel that Mieck claimed were primarily responsible for the

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The Massacre of St. Bartholomew's Day. This engraving simultaneously shows the assassination attempt on Admiral Coligny on August 22, his killing on August 24, and the massacre that followed. Center left: Coligny, on horseback, is shot as he reads a letter. Upper right corner: he is attacked in his bed, thrown from the window, and dragged off through the street below. Engraving reproduced courtesy of the Cabinet des Estampes, Bibliothèque nationale, Paris.

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These questions are difficult to answer. Memoirs and diaries give conflicting accounts. City hall registers are helpful but self-serving and incomplete, and the records of Parlement were destroyed on the orders of the king in the aftermath of the event.[3] A few official documents have survived, but the picture they yield is sketchy at best.[4] One avenue open to the historian who persists in seeking answers is to engage in a broader reconstruction of the background of the massacre. This is what I have attempted to do. By examining the behavior of the Parisian populace and the activities of those responsible for maintaining order during the fifteen years prior to St. Bartholomew's Day, I hope to establish a context that will cast new light on the climactic events of August 1572.

A chronological approach brings out the sequential patterns and cumulative effects of the religious strife. For analytical purposes, I have distinguished three stages in the conflicts that preceded St. Bartholomew's Day. The first, from 1557 to 1563, is characterized by the gradual breakdown of order in the city before and during the first religious war. The second period, 1563–67, is marked by a classic crisis of subsistence that reached its peak in the interval between the first two wars and contributed greatly to the climate of fear that existed when the second war broke out in 1567. The third period, 1567–72, is characterized by an intensification of the religious conflicts and an increasing difficulty in maintaining order. In the last phase we can trace a sequence of events that, if they did not in themselves lead directly to the massacre, nonetheless set the stage for it. The situation in Paris in 1572 was so volatile that the tiniest spark might have set it off.

SIXTEENTH-CENTURY OBSERVERS USUALLY EXAGGERATED the size of Paris. They put its population at four to five hundred thousand or more at mid-century, but scholars today find two to three hundred thousand a more reasonable estimate.[5]

massacre. Furthermore, I have not found support in archival sources for his argument that certain social groups in Paris made large profits from the confiscation of Huguenot properties in the 1560s or for his suggestion that there was a direct connection between these ill-gotten gains and the massacre of St. Bartholomew's Day. For work more useful in explaining the nature and causes of popular violence in the religious wars, see Natalie Zemon Davis, "The Rites of Violence," in Davis, *Society and Culture in Early Modern France* (Stanford, 1975), 152–88. For the debate between Davis and Estèbe, see *Past and Present*, 67 (1975): 127–35. Denis Richet also provided a useful discussion of the background to St. Bartholomew's Day; see his "Aspects socio-culturels des conflits religieux à Paris dans la seconde moitié du XVI^{e} siècle," *Annales: Economies, sociétés, civilisations*, 32 (1977): 764–89, esp. 770–72. John Salmon kindly brought to my attention the preface to Prosper Mérimée's *Chronique du règne de Charles IX*, written in 1829, which first popularized the idea that the massacre was the result of an unpredictable popular insurrection and not the consequence of a plot. Mérimée's insights were keen, even if his research on some points was faulty. Unfortunately, many historians have not noted his arguments.

[3] François Bonnardot *et al.*, *Registres des délibérations du Bureau de la Ville de Paris*, vols. 4–7 (Paris, 1888–93) (hereafter, *Reg. BV*). For the principal documents concerning St. Bartholomew's Day, see vol. 7: 7–23.

[4] A few documents relating to pillaging and other crimes of St. Bartholomew's Day survive in the minutes of Parlement. See Archives nationales (hereafter, AN), X^{2b}73, X^{2b}74. There are also some materials in the *sentences* of the Bureau de la Ville; AN, Z^{1h}70, Z^{1h}71. Also see a letter dated 2/10/72 (all dates are in the European style: day, month, year) in AN, H1881. Some records of the imprisonment of Huguenots in the days following the massacre can be found in the *registres d'écrou* of the Conciergerie, now in the Archives de la Préfecture de police (hereafter, AP), A^{B}4.

[5] In 1546 the Venetian ambassador, Mariano Cavalli, estimated the population of Paris at five hundred thousand. In 1561 Michele Suriano put it between four and five hundred thousand. M. N.

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Still, Paris was the largest city in Northern Europe, and its medieval walls enclosed a diverse and often turbulent population. The city's character was shaped by its role as a political capital, its scholarly and clerical institutions, and its importance as a trading center. Located near the confluence of navigable rivers that linked the heart of France to the Atlantic, Paris was a center for the import and transhipment not only of luxury items but also of bulky staples, such as grain, wine, and salt.[6] Provisioning the city required enormous quantities of these and other foodstuffs and provided employment for thousands of artisans and common laborers, who worked on the docks and in the public markets.[7] Many of the city's inhabitants were engaged in occupations easily disrupted by economic crises. Normally employed for scanty wages on a daily—or at best weekly—basis, these men and women became unemployed as soon as times got hard. They fought with one another for the few remaining jobs and begged for handouts alongside the city's traditional poor—the aged, orphaned, and infirm. In times of crisis, they were joined by thousands of outsiders, who poured into Paris to seek relief.[8]

Agents of the king and the municipality shared responsibility for keeping order and administering justice. The chief officers of the municipal government, the *prévôt des marchands* and *échevins*, had charge of policing the city's markets and ports. They also assumed certain police functions as a result of their responsibilities for tax collection, public works, and welfare, and they organized the bourgeois night watch and the local militia. While the citizens kept a standing watch in their *quartiers*, a professional guard accompanied on his rounds the knight of the watch (*chevalier du guet*).[9]

The functions we most commonly associate with police—the maintenance of public order and arrest of common criminals—were the primary responsibility of the agents of the *prévôté* of Paris (usually called the Châtelet). The *lieutenant criminel* of the Châtelet employed teams of officers who were responsible for the arrest, investigation, prosecution, and judgment of suspects. Cases tried in the Châtelet could be appealed to the high court of Parlement, and the judges of this court kept an eye on the administrative as well as the judicial functioning of the Châtelet, especially where matters of public order were concerned. The king and high

Tommaseo, *Relations des ambassadeurs vénitiens sur les affaires de France au XVI^e siècle*, 2 vols. (Paris, 1838), 1: 263, 483. H. J. Martin quoted the Italian Francesco d'Ierni who said that the population of Paris was three hundred fifty thousand in 1594 but had numbered six hundred thousand before the wars. See Martin, *Livre, pouvoirs et société à Paris au XVII^e siècle (1598–1701)*, 1 (Geneva, 1961): 34. On current estimates of the city's population in the sixteenth century, see my *Paris City Councillors: The Politics of Patrimony* (Princeton, N.J., 1983), xvi–xvii n.

[6] Emile Coornaert, *Les Français et le commerce international à Anvers: Fin du XV^e–XVI^e siècle*, 1 (Paris, 1961): 231–35, 242–44; Richard Gascon, *Grand commerce et vie urbaine au XVI^e siècle: Lyon et ses marchands*, 1 (Paris, 1971): 64, 66, 69, 93–94, 103–04, 129; and Michel Mollat, *Le commerce maritime normand à la fin du Moyen Age: Etude d'histoire économique et sociale* (Paris, 1952), 303–05, 314, 331.

[7] Richard Gascon, "La France du mouvement: Les commerces et les villes," in Fernand Braudel and Ernest Labrousse, eds., *Histoire économique et sociale de la France*, volume 1: *1450–1660* (Paris, 1977), pt. 1, pp. 257, 261.

[8] For a description of procedures for enrolling Parisian residents on relief, see Marcel Fosseyeux, "L'assistance parisienne au milieu du XVI^e siècle," *Mémoires de la Société de l'histoire de Paris et de l'Ile-de-France* (hereafter, *MSHParis*), 43 (1916): 117.

[9] On the overlap of authorities in Paris and their respective duties, see my *Paris City Councillors*, 3–32.

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officers of the crown also kept a close watch on Parisian affairs and intervened directly when they chose. A military governor, appointed by the king, was responsible for the city's external defense. When war threatened, he hired troops. The defense of the capital was too important to be left to the local militia.

Overlapping jurisdictions allowed inefficiency and conflict, corruption and misused influence, especially in the treatment of the Protestant minority. As early as 1559 we find accusations that officers of the Châtelet were sharing in the pillage of Protestant properties.[10] In 1560 Parlement investigated rumors that Châtelet officers were keeping confiscated properties that were supposed to have been turned over to charity.[11] And from 1561 on, accusations of extortion, battery, and theft by city authorities are common. So are accusations of false arrest and harassment.[12]

The Protestant community in Paris was always vulnerable to both official and popular persecution. A Reformed church was clandestinely established in the city in 1555, and, by the early 1560s, Protestant preachers were drawing large audiences in Paris as elsewhere in France. The reformers found converts among Parisians of all except perhaps the lowest social and economic groups.[13] The Parisian movement nonetheless failed to attain the numerical and political strength that the reformers acquired in other parts of the kingdom. Opposition was simply too strong. Although some members of the city's governing elite—more than is commonly thought—were initially attracted to the Reformed church, most abandoned it quickly when they found that religious deviance was incompatible with a career in the royal bureaucracy.[14] Other converts retired to their country estates or to Geneva. Those who remained in the city were very conscious of their minority status and seldom engaged in overt political activity. Parisian Protestants

[10] G. Baum and E. Cunitz, eds., *Histoire ecclésiastique des églises réformées au royaume de France* (hereafter, *Hist. eccl.*), 1 (1883–89; reprint edn., Nieuwkoop, 1944): 267–68.

[11] AN, $X^{2a}126$ (15/6/60).

[12] For example, a *sergent à verge* was convicted of extortion, battery, and theft; AN, $X^{2a}127$ (17/1/61). A *commissaire* of the Châtelet was accused of battery; AN, $X^{2b}34$ (8/10/62). A charge of false arrest was made against a *quartenier*; AN, $X^{2a}130$ (15/12/62). And a *sergent de bande* was accused of undue violence; AN, $Z^{1h}59b$ (30/6/63).

[13] The principal sources for the persecution of the Parisian Protestant community are the criminal registers and minutes of Parlement, 1562–72 (AN, $X^{2a}129$–140, $X^{2b}33$–73), and the registers of the Conciergerie (AP, A^B1–4), which have been published for the years 1564–68 and the first part of 1569. Also see Henri Bordier, "Poursuites et condamnations à Paris pour hérésie de 1564 à 1572 d'après les registres d'écrou de la Conciergerie du Palais," *Bulletin de la Société de l'histoire du protestantisme français* (hereafter, *BSHPF*), 50 (1901): 575–95, 639–53; and Nathanaël Weiss, "Huguenots emprisonnés à la Conciergerie du Palais à Paris en mars 1569," *BSHPF*, 72 (1923): 86–97. I am currently engaged in a quantitative analysis of these arrest records (AP, A^B1–4), which provide a valuable, if limited, insight into the sociology of Parisian Protestantism. At this point, the contours of Parisian Protestantism appear consistent with those of the movement in other urban milieus. See Benedict, *Rouen during the Wars of Religion*, 71–81; Joan Davies, "Persecution and Protestantism: Toulouse, 1562–1575," *Historical Journal*, 22 (1979): 31–51; Natalie Zemon Davis, "The Sacred and the Body Social in Sixteenth-Century Lyon," *Past and Present*, 90 (1981): 50–51; Garrisson-Estèbe, *Protestants du Midi*, 22–56; and Salmon, *Society in Crisis*, 133–38.

[14] Richet, "Aspects socio-culturels," 766–70. In my study of the Paris city councillors, I found that only three of the ninety men I investigated became Protestants, but another sixteen had close relatives (parents, children, siblings, or spouses) who adopted the new faith. I have since found eleven more men who had close relatives who became Protestants.

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never had the power to attempt the sort of coup that in 1562 put the Huguenots in control of the municipal governments of Lyons and Rouen.

The force of popular opposition also caused the Protestant movement in Paris to be more secretive than it was in many provincial towns. With the exception of a brief period in the fall and winter of 1561–62, Protestants were never allowed to hold public religious services in the city, and, in the decade between the first civil war and St. Bartholomew's Day, even clandestine services were only rarely celebrated.[15] We cannot measure the strength of the Protestant community in Paris, as we can that of certain provincial communities, from membership lists, baptismal records, and other church documents.[16] Parisian Protestants either worshiped privately or fled the city and worshiped elsewhere. Only those who were arrested left a trace in the records. Judicial records and narrative accounts nevertheless suggest that the popular appeal of the movement in Paris peaked as early as 1561–62. Successive waves of defection from Protestantism accompanied each of the first three religious wars, and another, massive wave followed St. Bartholomew's Day. These facts were not, however, necessarily evident to Parisian Catholics. They did not measure the threat posed by the Protestant heresy in terms of numbers of converts—something of which they were only vaguely aware. Rather, in their minds, the heresy was an infection that polluted the social order; it appeared to be spreading and had to be eliminated.

A full account of the breakdown of order in Paris must begin in 1557, when the French, after the disastrous defeat of their army outside St.-Quentin, feared that the Spanish would end the lengthy Hapsburg-Valois wars by taking Paris. The fears generated by the defeat and the hasty preparations for the city's defense contained elements that surfaced later in the religious wars—among them a nascent animosity toward Gaspard de Coligny, who, along with his uncle Anne de Montmorency, was held responsible for the defeat, and a growing loyalty to the duc de Guise, who salvaged France's position by subsequent victories at Calais and Thionville.[17]

Closely following the defeat at St.-Quentin, and somehow entwined with it in the popular imagination, was the first of many large-scale public attacks in Paris on followers of the Reformed faith.[18] A furious crowd assaulted a group of Protestants who had gathered for worship in a house in the rue St.-Jacques. In the years that follow, reports of attacks on and official persecution of suspected Protestants multiply. So do reports of Catholic preachers stirring their listeners to violence.[19] It is well known that Henry II, when he made peace at last with Philip

[15] Nathanaël Weiss, "Lieux d'assemblées huguenotes à Paris avant l'Edit de Nantes, 1524–1598," *BSHPF*, 48 (1899): 152–53.

[16] See, for example, Benedict, *Rouen during the Wars of Religion*, 71–81, 251–55.

[17] *Reg. BV*, 4: 494–501; and François Grin, "Journal," ed. A. de Ruble, *MSHParis*, 21 (1894): 16.

[18] For accounts in which a connection is made between the defeat at St.-Quentin and the affair of the rue St.-Jacques, see *Hist. eccl.*, 140; Etienne Pasquier, *Oeuvres*, 2 (Amsterdam, 1723), 75–76; and "A propos de l'affaire de la rue Saint-Jacques (4–5 septembre 1557)," *BSHPF*, 96 (1947): 98. On the affair of the rue St.-Jacques itself, see Nathanaël Weiss, "Episodes de la Réforme à Paris: L'assemblée de la rue St.-Jacques," *BSHPF*, 65 (1916): 195–235; and *Hist. eccl.*, 139–45.

[19] On Catholic preaching in 1558 and 1559, see the letters of Macard to Calvin, dated 27/3/58, 22/5/58, and 21/3/58[?], in Athanase Coquerel, *Précis de l'histoire de l'Eglise Réformée de Paris* (Paris, 1862),

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of Spain, intended to return home to take up the battle against heresy and that, on his death, the Guises used their considerable influence at court to intensify this fight. A series of important arrests were made, and it was hoped that the whole underground network of Parisian Protestantism might be uncovered.[20]

Public execution of condemned heretics served as a warning to the Parisian crowds who gathered to watch. Burning the condemned alive was one of the more horrible forms of execution, traditionally reserved for such grave crimes as heresy.[21] Despite, or perhaps because of, its horrors, public burning seems often to have stirred the passions of the crowds, and incidents in which the charred bodies of executed Protestants were torn down by the populace, dragged through the streets, and subjected to further atrocities were not uncommon.[22] In the famous case of Anne du Bourg, a counselor in Parlement executed for heresy in December 1559, the crowds were so agitated that they would have snatched the condemned man on the way to the execution site and taken charge of his execution themselves, had they not been stopped by the numerous guards around him.[23] At other executions the guards were less numerous and had difficulty controlling the angry crowds. Although ordinances were issued against interference with executions and the proper disposal of the corpses of those executed, the crimes continued.[24]

RELIGIOUS UNREST IN PARIS posed serious problems for Catherine de Medici. After the death of François II in December 1560, she adopted a more conciliatory policy toward the Protestants as a means of holding together the state for her young son Charles IX. A royal edict of April 19, 1561, ordered the cessation of all religious violence and forbade religious persecution.[25] The edict, however, brought little peace to the city. Attacks by Catholic crowds on persons suspected of attending

xxxvii, xl, lxxiii; *Hist. eccl.*, 168, 192–94; and AN, $X^{2a}125$ (31/5/60). For the best discussion of Catholic polemic in the prewar period, see G. Wylie Sypher, "'Faisant ce qu'il leur vient à plaisir': The Image of Protestantism in French Catholic Polemic on the Eve of the Religious Wars," *Sixteenth Century Journal*, 11 (1980): 59–84.

[20] On the persecutions of 1559, see *Hist. eccl.*, 263–75, 281–84. Parliamentary records include the cases of Marguerite Le Riche (AN, $X^{2a}124$ [19/8/59]) and Jean Geoffroy ($X^{2a}125$ [7/12/59]), ($X^{2a}918$ [7/12/59]). In both cases, a principal aim of the judges was to induce the defendants to reveal the names of their "accomplices" in heresy. Denis François Sécousse, ed., *Mémoires de Condé*, 6 vols. (London, 1743), 1: 305–15, 539–55; *Reg. BV*, 5: 47–48, 52–53; Regnier de la Planche, *Histoire de l'Estat de France*, in J. A. C. Buchon, ed., *Choix de chroniques et mémoires*, 1 (Paris, 1836): 220–26, 236–37.

[21] I suspect that the judges of Parlement who ordered prisoners to be burned alive had some qualms about the cruelty of the sentence, for, in many cases (including those of Le Riche and Geoffroy), a secret clause was appended to the sentence ordering the victim strangled before feeling the flames. For a discussion of this practice, see Alfred Soman, "La justice criminelle aux XVI^{e}–XVII^{e} siècles: Le Parlement de Paris et les sièges subalternes," *107^{e} Congrès national des Sociétés savantes (Brest, 1982), philologie et histoire jusqu'à 1610*, 1 (Paris, 1984): 30–35.

[22] For example, see "Journal de ce qui s'est passé en France durant l'année 1562," *Revue rétrospective*, ser. 1, vol. 5 (1834): 101; *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 95. For different accounts of a single incident, see AN, $X^{2a}131$ (28/6/63); *Reg. BV*, 5: 236–37; *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 130; and *Despatches of Michele Suriano and Marc Antonio Barbaro*, Publications of the Huguenot Society of London, vol. 6 (Lymington, 1891), 102.

[23] De la Planche, *Histoire de l'Estat de France*, 236.

[24] AN, $X^{2a}131$ (28/6/63), $X^{2a}133$ (29/2/65).

[25] For the edict of April 19, 1561, see Sécousse, *Mémoires de Condé*, 2: 332.

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Protestant services continued; so did agitation stirred up by seemingly trivial forms of harassment—for example, by young boys parading through the streets carrying crosses and saints' images and forcing passers-by to genuflect.[26] Although the edict prohibited the holding of Protestant services, many Catholics interpreted it to mean that "the fullest liberty was given to everyone to profess the religion he chose," and Parisians were outraged by the king's apparent willingness to tolerate heresy.[27] The officers of Parlement drew the same meaning from the edict, which seemed, by its refusal to countenance religious persecution, to approve "religious diversity," and they sent stern remonstrances to the king.[28]

Because of the complaints, another edict was issued in July that explicitly renewed old prohibitions on Protestant assemblies and forbade the administration of the sacraments according to any but the traditional forms.[29] Still, the Catholics were not satisfied. The new edict promised freedom of conscience and forbade persecution for what people did in their own homes. In the opinion of Parisian Catholics, these clauses made the provisions of the edict unenforceable.[30]

It is true, moreover, that even these limited concessions encouraged the spread of Protestant preaching. Etienne Pasquier reported that eight or nine thousand people attended Protestant services outside the porte St.-Antoine in October 1561, and, on All Saints' Eve, a high noble family openly held services in their house in Paris in defiance of the edict of July.[31] The Protestants claimed that Catherine had secretly given them permission to assemble, either in private houses or outside the city wall, and had ordered the city guard to protect the assemblies.[32]

Parisian Catholics were infuriated by the insolence of the Huguenots (who, they claimed, wore arms to Protestant mass assemblies) and by the protection the heretics seemed to enjoy. The Catholic memoirist Nicolas Brulart undoubtedly expressed a common resentment when he wrote that the Huguenots held most of the city "in subjection."[33] Catholic preachers raged against the Huguenots, urging their parishioners to take up weapons to defend their churches from the heretics. They even challenged the authority of the crown. A Carmelite preaching at St.-Benoît reportedly compared the situation in the kingdom to that of two children playing with an apple who were about to let it be snatched by a third.[34] Another cleric, in a public debate at the Sorbonne, defended the proposition that

[26] The most serious incident was the riot that took place on the pré aux Clercs on April 27, 1561. See *ibid.*, 2: 341–47; *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 26–27; and AN, $X^{2a}128$ (8/5/61), (21/6/61), and (7/8/61). A notation in the *plumatifs* records that all chambers assembled on April 28, 1561, to hear the king's edict regarding the pré aux Clercs; AN, $X^{2a}920$. On the practice of parading crosses through the streets, see Sécousse, *Mémoires de Condé*, 2: 369; and de la Planche, *Histoire de l'Estat de France*, 236–37.

[27] *Despatches of Michele Suriano*, 24.

[28] For the remonstrances of Parlement regarding the king's letters of April 19 and 20, 1561, see Bibliothèque nationale (hereafter, BN), MS. Dupuy 722.

[29] *Edict du Roy sur le faict de la Religion, publié en la cour de Parlement, le dernier jour de juillet, 1561* (Paris, 1561); and Pasquier, *Oeuvres*, 85–86.

[30] *Despatches of Michele Suriano*, 32–33.

[31] Pasquier, *Oeuvres*, 87–88.

[32] *Hist. eccl.*, 740–41.

[33] *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 67.

[34] Sécousse, *Mémoires de Condé*, 2: 532; and BN, MS. fr., N.a. 8061, f. 35r.

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the pope had the power to excommunicate a king and free his subjects from their obligation of loyalty if the king was found to favor heretics.[35] Ordered to look into these and other incidents, Parlement by the end of November was investigating reports of seditious preaching not only at the Sorbonne but also in some of the most important parishes in the city—St.-Benoît, St.-Severin, St.-Germain-de-l'Auxerrois, St.-Eustache, St.-Merry, and St.-Jacques-de-la-Boucherie.[36]

Catherine left investigation of most of these reports to Parlement, but rumors of the seditious character of the Advent sermons preached by a friar of the Minim order in the parish of St.-Barthélemy led to more direct action. During the night of December 9, the friar, Jean de Hans, was summarily arrested and dragged off to the court at St.-Germain. Parisians were outraged. The Hôtel de Ville sent a group of prominent merchants to plead with the king for the return of "their" friar, while Parlement ordered the city guard and the Châtelet officers to calm the excited crowds and tell them that the cleric was taken on the orders of the king. Meanwhile, the king dispatched troops to prevent a full-scale riot in the city and freed de Hans, who was escorted triumphantly into Paris by the delegation of merchants. The following day the parishioners of St.-Barthélemy organized a procession to thank God for this sign of his favor.[37]

Two weeks later the fragile peace that prevailed in the city was shattered by a violent clash between the Huguenots worshiping in the faubourg St.-Jacques at a house known as the Patriarche and the Catholics of the neighboring church of St.-Médard. It is hard to know who touched off the melee. The best reconstruction of the event suggests that the Protestants, whose services were interrupted by the tolling bells of St.-Médard, sent envoys to ask that the bells be silenced. The Catholics did not immediately agree, and an argument ensued. When more men came over from the Patriarche to find out what was happening, the parishoners of St.-Médard thought they were being attacked, and a battle broke out that resulted in serious injuries—even deaths, the Catholics claimed—and in acts of sacrilege and pillage. Heads were knocked off the statues of saints, crosses were broken, and holy chalices stolen or defiled.[38] I shall not attempt here to reconcile the conflicting accounts of the event. What is important is that the Catholics of St.-Médard sincerely believed they had been attacked. Just as their preachers had

[35] BN, MS. fr., N.a. 8061, ff. 15–17, 25–26, 35, 39, 43, 51–55; and Pasquier, *Oeuvres*, 89–90. Pasquier apparently confused this preacher, whose name was Tanquerel, with Jean de Hans.

[36] Sécousse, *Mémoires de Condé*, 2: 285–88; BN, MS. fr., N.a. 8061, ff. 11, 12; and Catherine de Medici, *Lettres*, ed. Hector de la Ferrière and Gustave Baguenault de la Puchesse, 11 vols. (Paris, 1880–1943), 1: 182.

[37] AN, X^{2a}1599 (10/12/61) and (12/12/61); *Reg. BV*, 5: 109 (13/12/61), 109 n.; Claude Haton, *Mémoires*, ed. Félix Bourquelot, 2 vols. (Paris, 1857), 1: 210–13; and Pasquier, *Oeuvres*, 89–90. Also see Alphonse de Ruble, "L'arrestation de Jean de Hans et le tumulte de Saint-Médard," *Bulletin de la Société de l'histoire de Paris et de l'Ile-de-France*, 13 (1886): 85–94.

[38] For Protestant accounts, see *Hist. eccl.*, 745–51; Sécousse, *Mémoires de Condé*, 2: 541–57; "Histoire véritable de la mutinerie, tumulte et sédition faicte par les prestres de Sainct-Médard," in L. Cimber and F. Danjou, eds., *Archives curieuses de l'histoire de la France*, 27 vols. (Paris, 1834–40), 4: 51–68; and Great Britain, Public Record Office, *Calendar of State Papers, Foreign* (hereafter, *CSPF*), vol. 4, nos. 783 (4/1/62), 789. For related documents, see Cimber and Danjou, *Archives curieuses*, 4: 63–102. For Catholic accounts, see *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 68–69; Grin, "Journal," 21; Jean de La Fosse, *Journal d'un curé ligueur de Paris sous les trois derniers Valois*, ed. Edouard de Barthélemy (Paris, 1866), 45; and *Discours sur le saccagement des églises catholiques par les hérétiques* (n.p., 1562), ff. 31–35.

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predicted, their church had been invaded by Huguenots and the things they held sacred defiled.

Not surprisingly, in the aftermath of the event, other parishes in the city demanded arms to protect themselves.[39] Furthermore, the Catholics believed that special guards, recently mustered to help keep peace in the city, had taken part in the battle on the Huguenot side, and they were scandalized to learn that no Huguenots had been arrested but that more than thirty Catholics—including six priests—were prisoners of the Châtelet.[40] Although the prisoners were released two days later and a full investigation ordered by Parlement,[41] Catholics saw the incident as a blatant example of Huguenot brutality and proof of their growing power. Brulart wrote that there were "three or four thousand" armed men milling about in the streets near St.-Médard, "holding in subjection" the local residents, and a Catholic tract published in 1562 describes Huguenots parading through the streets in battle order, crying, "The Evangile, the Evangile, where are the idolatrous papists?"[42]

The St.-Médard affair was still much in the public's mind when, on January 17, 1562, a new edict of toleration was announced.[43] Although the edict again forbade Protestants to assemble inside city walls, Catholics found it unacceptable. The city council, the clergy, and the doctors of the Sorbonne all protested the edict, as did a special delegation of Parisian merchants, acting on their own initiative.[44] Parlement also remonstrated with the king and only registered the edict on March 6, after having twice received *lettres de jussion*. Parlement was subjected to other pressures in addition to the king's displeasure. On March 4, about four hundred armed men had gathered in the courtyard of the palace to protest the delay in the edict's registration. Brulart and other memoirists considered this attempt at intimidation an important factor in Parlement's eventual decision to register the offensive decree.[45]

Given these circumstances and the unrest they provoked, it is hardly surprising that Catholics greeted the duc de Guise with great enthusiasm when he marched into Paris on March 16, fresh from the massacre of Huguenots gathered for worship at Vassy. A military hero as well as a staunch Catholic, Guise was welcomed as the man who would put an end to Huguenot insolence.[46] His arrival was well

[39] Michel Félibien and Guy-Alexis Lobineau, *Histoire de la ville de Paris*, 5 vols. (Paris, 1725), 2: 1079.

[40] *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 169; *Discours sur le saccagement*, f. 31; and Pasquier, *Oeuvres*, 89–90. Indignation over the use of officers of the guard, who were paid by the Parisian bourgeoisie, to protect the Protestant assemblies is also shown in *Remonstrances faictes au roy de France par Messieurs de la Court de Parlement de Paris sur la publication de l'Edict du Moys de Janvier* (Cambrai, 1562).

[41] AN, X^{2a}129 (29/12/61).

[42] *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 68; and *Discours sur le saccagement*, f. 31.

[43] André Stegmann, ed., *Edits des Guerres de Religion* (Paris, 1979), 8–14.

[44] "Un manifeste parisien contre le premier Edit de tolérance (1562)," *BSHPF*, 17 (1868): 534–40. The city registers make it clear that the edict was not an official manifesto drawn up by city officials; *Reg. BV*, 5: 117.

[45] *Remonstrances faictes au roy*; *CSPF*, vol. 4, nos. 849 (28/1/62), 930 (9/3/62); *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 73; and "Journal de ce qui s'est passé," 81, 84.

[46] *CSPF*, vol. 4, no. 943 (20/3/62).

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staged. He deliberately chose to enter through the porte St.-Denis, the gate used for royal entries, in order to give an impression of power and authority. And, indeed, he was greeted royally. He was met by an impressive entourage of nobles, city officers, and bourgeois. The crowds that lined the streets to view his arrival shouted their joy—and their hatred of the Huguenots.[47]

In the weeks that followed, tensions in Paris reached new heights. A bloody riot took place on March 20, when a Catholic crowd attempted to disinter a corpse that had been buried according to Reformed rites in the Cemetery of the Innocents.[48] The presence of the Protestant leader Louis de Bourbon, prince de Condé, who was in Paris to seek redress for the Vassy massacre when Guise arrived, aggravated the situation. Condé's followers, as well as those of Guise, had flocked to the city.[49] Easter week, which began on March 22 with the traditional Palm Sunday processions, provided numerous occasions for clashes between the rival factions. Pasquier described the processions as a scene of "chaos and confusion" in which "pistol shots and cannons served as carillons."[50] An anonymous memoirist similarly observed, "One heard so often the report of firearms that it seemed Paris was a frontier town."[51]

The confrontation that Parisians expected was prevented by the city's new governor, the cardinal of Bourbon, who persuaded both Condé and Guise to leave. Condé left on the twenty-third, retiring to one of his properties. Guise left on the following day. He went with his allies of the Triumvirate to Fontainebleau, where he took the king under his protection—or, as Condé saw it, into captivity.[52] Most historians have concluded that Condé's departure and Guise's action set the terms for confrontation and made war inevitable. As Pasquier pointed out, Condé placed himself at a grave disadvantage when he abandoned not only the king but also the capital to his enemies.[53]

Pasquier had in mind the long-term consequences of Condé's action, which made the Huguenots appear to be rebels against the crown, but it is equally true that in the short term his departure cleared the way for bloody reprisals against the Huguenots still in Paris. On April 4, Constable Montmorency, another member of the Triumvirate, came back to Paris from the court and, accompanied by his soldiers, proceeded to destroy the two principal sites of Reformed preaching in the Paris suburbs. To the great delight of the Parisian populace, he made a bonfire of the pulpit and benches found in the house known as Jerusalem in the faubourg St.-Jacques. Then he did the same at a house in the northeastern suburbs

[47] Jacques-Auguste de Thou, *Histoire universelle*, 16 vols. (London, 1734), 4: 172.
[48] "Journal de ce qui s'est passé," 85. La Fosse related a similar event but dated it March 15; *Journal d'un curé ligueur*, 45.
[49] *CSPF*, vol. 4, no. 943 (20/3/62). For Condé's own explanation of his actions, see his *Declaration faicte par Monsieur le prince de Condé pour monstrer les raisons qui l'ont contrainct d'entreprendre la defense de l'authorité du Roy...* (n.p., 1562).
[50] Pasquier, *Oeuvres*, 95–96.
[51] "Journal de ce qui s'est passé," 87.
[52] Condé, *Declaration faicte par Monsieur le prince de Condé.*
[53] Pasquier, *Oeuvres*, 95–96.

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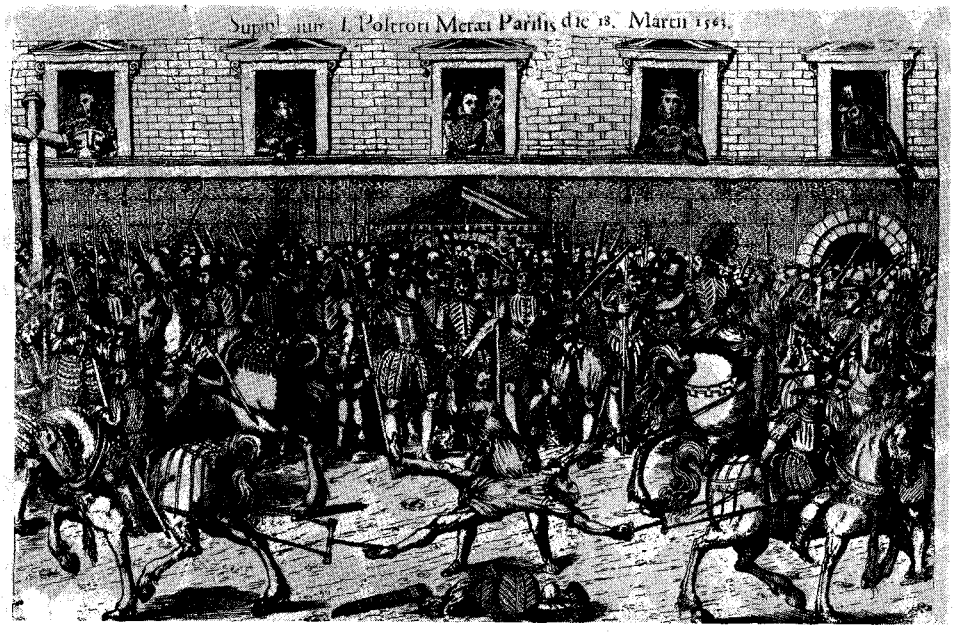
This scene from a 1562 Catholic polemic against the "Horrible Cruelties of the Huguenots in France" shows a priest being forced to profane the mass he is celebrating. In a simultaneous depiction, the same priest is being beaten, and he is being shot as he hangs from the crucifix over the altar of his church. Engraving reproduced courtesy of the Cabinet des Estampes, Bibliothèque nationale, Paris.

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Another plate from the same polemic shows some of the cruel tortures the Huguenots were said to inflict on their Catholic enemies. Engraving reproduced courtesy of the Cabinet des Estampes, Bibliothèque nationale, Paris.

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The execution of Jean Poltrot, the assassin of the duc de Guise. On March 18, 1563, before a large crowd on the place de Grève, Poltrot was first seared by red-hot pincers and then drawn and quartered. This engraving, one in a series of engravings of massacres by J. Torterel and J. Perrissin, is reproduced courtesy of the Cabinet des Estampes, Bibliothèque national, Paris.

known as Popincourt. The next day, a Sunday, a great crowd gathered at Popincourt to complete the destruction. They tore the house into pieces and brought the rubble into the center of the city, to the place de Grève, where, to jubilant cries that "God has not forgotten the people of Paris," they set fire to it.[54]

On April 6, the king was brought back to the city. Parisians talked of nothing but war, and, in the ensuing days and weeks, preparations were made for the city's defense. People had already taken up arms during Easter week, but now a new militia was organized and money raised for hiring troops.[55] There was a heady combination of fear and anticipation. The Parisians were certain that Condé was going to attack. When Catherine decided on May 13 to take the young king back out of Paris, no one slept at night for fear of being surprised by the Huguenots. On May 16, a noisy quarrel between a couple of boatmen at four in the morning caused a panic. Thinking a Huguenot army was at the city gates, people grabbed up their arms, dragged heavy chains across the streets, and prepared to fight.[56]

On May 26, all Protestants were ordered to leave Paris within forty-eight hours.[57] Many had already chosen to leave; others had remained in order to protect their property or because they had no place to go. The captains of the city's newly formed militia were charged with identifying the individuals in each *quartier* who

[54] *Ibid.*; "Journal de ce qui s'est passé," 91–92; Sécousse, *Mémoires de Condé*, 3: 198–99; *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 80–81; and La Fosse, *Journal d'un curé ligueur*, 46–47.

[55] Pasquier, *Oeuvres*, 95–96; "Journal de ce qui s'est passé," 93; Sécousse, *Mémoires de Condé*, 3: 419–21; and *Reg. BV*, 5: 120–23.

[56] "Journal de ce qui s'est passé," 105–06.

[57] Félibien and Lobineau, *Histoire de la ville de Paris, Preuves*, 1: 667.

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were "notorious" for their religious beliefs. In some *quartiers* at least, the inhabitants were called together in special assemblies to denounce suspicious persons, who were then ordered to sign a formal confession of faith or leave the city immediately.[58]

As rumors reached Paris of Protestant victories elsewhere in France, all semblance of order broke down. In late June, some Huguenots pillaged Catholic churches and houses in the neighboring town of Meaux. The king's lieutenant and Parlement declared the perpetrators guilty of *lèse-majesté* and rebellion and subject to death. Many people took this declaration as permission to attack anyone suspected of practicing the new religion. Reputed Huguenots were struck down in the streets. Sometimes mock trials were held; the attackers grilled the captives on their religious beliefs and, when not satisfied with the answers, killed them on the spot.[59] Officials who tried to intervene were themselves in danger, and edicts against violence were bitterly protested. As one anonymous memoirist described it, "The people wanted nothing less than permission to kill and exterminate the Huguenots without any form of trial; but the consequences were too dangerous."[60] He implied that permission might have been given, had it been possible to contain the violence.

Public disturbances, and tenuous attempts to control them, continued through the summer and fall of 1562. The fighting did not reach the Paris region until November, and, although the approach of the rival armies caused new fears among the populace and arrests for religious deviance became more numerous, the persecution of accused Huguenots could scarcely have become worse.[61]

Parisians violently opposed the peace treaty concluded at Amboise on March 19, 1563—their opposition intensified by the assassination of the duc de Guise by a Huguenot in February. Only with great difficulty had city officials maintained order after Guise's death, and observers remarked on the ominous mood of the crowds that came to watch the execution of his assassin. The display of grief at Guise's funeral was awesome. The Parisians wanted vengeance; they did not want peace.[62] Two princes of the blood came to the capital to oversee the registration of the edict of pacification—and to stop the popular tumult that was expected.[63]

The peace was announced, but enforcing it was another matter. On April 10, the king reprimanded the city for continued violence and a general refusal to allow Parisian Huguenots to return to their homes and work, as they had been promised in the edict of pacification.[64] Violent incidents and conflicts over property

[58] On local assemblies, see AN, $X^{2b}34$ (7/11/62); and "Journal de ce qui s'est passé," 111.

[59] *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 90–91; "Journal de ce qui s'est passé," 172–73; and La Fosse, *Journal d'un curé ligueur*, 52.

[60] "Journal de ce qui s'est passé," 173.

[61] Many arrest records survive for this period; AN, $X^{2b}34$, $X^{2a}130$. Often the Huguenots cited personal enmities as the reason for their arrests, but in several cases they were arrested to rescue them from angry crowds. See, for example, the case of Nicholas Delerigny, in AN, $X^{2b}34$ (13/1/62). Also see "Journal de ce qui s'est passé," 212.

[62] Catherine de Medici, *Lettres*, 1: 517–18; *Despatches of Michele Suriano*, 79–84; and Grin, "Journal," 21–27.

[63] *Despatches of Michele Suriano*, 85.

[64] *Reg. BV*, 5: 212–14.

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continued through the spring and summer. The king's orders for the militia to lay down arms and disband caused particular outrage. Only in the autumn did an air of relative tranquillity seem to settle over the city.[65]

DURING THE NEXT FEW YEARS, crises in Paris concerned food, not religion. Even while the first religious war was being fought, a subsistence crisis developed that was more severe than any that had occurred in over a century.[66] Observers, such as Claude Haton, believed the natural disasters that beset the Paris basin during the religious wars were signs of God's wrath. In 1562 the Parisians were punished for their "pride and sins" by a summer so cold and wet that the crops rotted in the fields.[67] In addition to the wars, the reversal of the seasons, and the consequent dearth, there was another sign of God's anger—pestilence. According to Haton, the plague raged in Paris for over a year and caused more than twenty-five thousand deaths. Other sources corroborate his testimony to the seriousness of the epidemic.[68]

During the first war, then, Parisian authorities had to cope not only with religious conflict but also with scarcity and contagion. By December 1562, grain prices, which were already unusually high at the start of the war because of previous bad harvests, reached the unprecedented level of nine *livres* per *setier*, more than double the average prewar price.[69] In addition to losses from bad weather and plague, which kept some of the grain from reaching the markets, troops quartered in the grain belt around Paris pillaged stores.[70]

Prices eased after the war, and it appeared they would return to prewar levels. In 1564, however, a sequence of natural disasters began again. Part of the new crop was destroyed by rain before it could be brought in from the fields. Then came a winter so cold the wine froze in the cellars and peasants were found dead in their fields. François Grin called this the "year of three winters." The rivers, which were such solid ice in December that a man could walk across them, flowed furiously when they finally thawed.[71] The Seine overflowed its banks, and it was a long time before the river was safely navigable and provisions could be brought into the city.[72] The cold spring meant that the 1565 harvest would be poor, and by June the king had issued orders to prohibit the export of grain. By late August, when

[65] AN, X^{2a}130 (25/5/63) and (5/6/63), X^{2a}131 (28/6/63), Z^{1h}59b, ff. 82v, 101v, 107r, 119–22, 123v-24; *Reg. BV*, 5: 219, 236–37, 248, 253, 256–57; *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 130, 132; Sécousse, *Mémoires de Condé*, 2: 160; and La Fosse, *Journal d'un curé ligueur*, 64–65.

[66] *Despatches of Michele Suriano*, 35; and Micheline Baulant and Jean Meuvret, *Prix des céréales extraits de la mercuriale de Paris (1520–1698)*, 1 (Paris, 1960): 49, 152–53.

[67] Haton, *Mémoires*, 1: 332–33.

[68] *Ibid.*; and Léon Brièle, *Collection de documents pour servir à l'histoire des hôpitaux de Paris*, 4 vols. (Paris, 1881–87), 1: 6, 3: 317–18.

[69] Baulant and Meuvret, *Prix des céréales*, 156.

[70] Brièle, *Collection de documents*, 1: 6, 3: 317.

[71] Haton, *Mémoires*, 1: 391–97; and Grin, "Journal," 28.

[72] *Reg. BV*, 5: 492.

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the price of wheat reached eleven *livres* per *setier*, the city was actively engaged in attempts to import supplies from places as distant as Sweden and Danzig.[73]

During the grain crisis, city officials feared sedition. Municipal records reveal that the prudent city fathers wanted to supply grain to the needy so that the needy would not try to help themselves to supplies they believed were stored in the houses of the rich. "Not finding any," the officials believed, the poor "would use violence on the properties and persons of the weak, which would be a beginning of riot and sedition that could only be appeased by arms."[74]

Attempts to import grain were frustrated by the extent of the area affected by scarcity and the difficulties of transport. An early freeze in the north meant that the promised grain from Danzig could not be delivered before the next spring.[75] In the meantime, the city tried to cope with a great influx of paupers. Rules for the policing of the poor were tightened, and a tax was levied on the bourgeoisie to pay for alms.[76]

Grain prices remained high, averaging nine or ten *livres* per *setier*, right through the spring of 1566.[77] The promise of a good harvest caused tensions to ease a bit, but prices had not yet begun to fall noticeably when torrential rains once again wiped out the crop. In July the price of wheat topped twenty-one *livres* per *setier*—more than five times the average price in any period before 1561.[78] The danger of a grain riot appeared acute. Carts were burned on their way to market, and, in order to protect the bakers, who were inevitably blamed for the high price of bread, the city had to station armed guards in the markets of the popular *quartiers* of the city.[79] Convents, barns, and private homes were searched for hidden stores, and officials were sent out to commandeer grain from the countryside in order to provision the capital.[80] By August sufficient grain had been procured to cause prices to begin to drop, but the crisis was not really over. Prices remained at a level roughly double the prewar average.[81]

The significance of this crisis is twofold. First, it is important to recognize that the period between the wars was one of merely relative calm and stability. In fact, all of the socioeconomic tensions already present in the city were aggravated. Second, the crisis fueled popular hatreds as the second religious war broke out in the autumn of 1567.

The Protestants, irritated by the crown's failure to enforce the edict of pacification, brought on the second war by their plan to seize the king at Meaux. When the plot was discovered, they turned on Paris, to which the king had

[73] *Ibid.*, 5: 515–33; and Baulant and Meuvret, *Prix des céréales*, 51–52, 170–78.
[74] *Reg. BV*, 5: 518–20.
[75] *Ibid.*, 5: 537.
[76] *Ibid.*, 5: 536, 550 n.
[77] Baulant and Meuvret, *Prix des céréales*, 176.
[78] *Ibid.*, 177; Grin, "Journal," 34; and Haton, *Mémoires*, 1: 410.
[79] *Reg. BV*, 5: 564.
[80] *Ibid.*, 5: 564–65 n.
[81] Baulant and Meuvret, *Prix des céréales*, 52–53.

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returned.[82] While Condé and the king engaged in strained negotiations, Huguenot forces invested the city. On the night of October 1, the Huguenots burned a dozen or more windmills outside the porte St.-Denis. The flames could be seen clear across the city, and the Parisians, thinking they had been attacked, rushed to take up their weapons, which had been returned to them two days earlier.[83] A rumor that the Huguenots had accomplices within the walls—prepared to set fire to their own houses in order to burn the city—increased the fear.[84]

The following days were chaotic. Many of the specific elements present in the St. Bartholomew's Day massacre—both symbols and targets—began to appear. According to Pasquier, anyone who did not put the sign of a white cross on his hat was in danger of being killed.[85] Five years later, white crosses marked the Catholic crowds on St. Bartholomew's Day. An anonymous Catholic observer confirmed that the symbol of the white cross emerged in the riots of 1567. Furthermore, he added, "It was permitted, in these circumstances, following the counsel of Christ, to sell one's tunic and purchase a sword. . . . If someone said a word in favor of the authors of the rebellion, it was permitted to kill him, which was the fate of many."[86]

The Huguenots did not try to enter the city. The firearms, rocks, and chains the Parisians had prepared for their defense were not needed. A new fear, however, arose. The Huguenots had cut off the entrances to the city, and they were prepared to starve it into submission. Paris had not yet recovered from the interwar scarcity, and this new threat to the grain supply drove the Parisians into a frenzy.[87] Their anger increased through the month of October, as the futile negotiations droned on and the shortage of foodstuffs became acute. According to Jacques-Auguste de Thou, if the king had not been in the city, there would have been an uprising.[88]

The threat of popular violence eventually pushed Constable Montmorency, in charge of the defense of the capital, to take the offensive. His troops made several sorties, which allowed the city to be provisioned, although, again according to de Thou, "the people did not on that account cease to murmur."[89] On November 10, Montmorency finally led his army to battle. The aged constable received mortal wounds that day on the plain of St.-Denis, but the battle itself was not decisive. Although the Huguenots gave up their plan of starving the Parisians into submission, the war continued until March 1568. At that point the Huguenots' position was sufficiently strong for the peace to be concluded on terms favorable to their party.

[82] Catherine de Medici, *Lettres*, 3: 20 n., 60 n., 62 n.; Pasquier, *Oeuvres*, 117–18; and Félibien and Lobineau, *Histoire de la ville de Paris, Preuves*, 1: 694.

[83] De Thou, *Histoire universelle*, 5: 354–59; and Henri Hauser, "Un récit catholique des trois premières guerres de religion: Les *Acta Tumultuum Gallicanorum*. . . ," *Revue historique*, 108 (1911): 303–04.

[84] Hauser, "Un récit catholique," 303.

[85] Pasquier, *Oeuvres*, 117–18. On the meaning of white crosses, see La Fosse, *Journal d'un curé ligueur*, 89.

[86] Hauser, "Un récit catholique," 303–04.

[87] *Ibid.*, 304–05; Pasquier, *Oeuvres*, 119–22; and Grin, "Journal," 41.

[88] De Thou, *Histoire universelle*, 5: 367.

[89] *Ibid.*, 5: 367–68.

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The Parisian populace bitterly resented the concessions made in the Peace of Longjumeau.[90] An anonymous poem in the form of an echo sums up the popular sentiment in Paris in the spring of 1568: "Pour traicter ceste paix, que gaigne notre Royne?" And the echo: "Hayne."[91] The Parisians' resentment was encouraged by Catholic preachers, who raged against the peace in their Lenten sermons. As recounted by Haton, the sermons predicted dire consequences for the king if he did not cease to support false prophets. Haton insisted that the preachers did not urge the people to rebellion, but they did warn that the Huguenots would destroy France, "if they were not exterminated by force of arms."[92]

The troubled Peace of Longjumeau could not last. Neither side disarmed. The third war, which began in August 1568, was in effect a continuation of the second.[93] Although Paris was not a major scene of the fighting, tensions in the city remained high. In November 1568 city officers were sufficiently frightened of a Huguenot attack to order all the houses outside the northern gates torn down, so that the enemy could not find shelter in the shadow of the walls.[94] Emergency measures were put into effect, and regular searches were made for Huguenots and hidden arms.

It is clear throughout this period, however, that a Huguenot attack was not the only danger that worried city authorities. All through the second and third wars, they struggled to prevent mob violence. Ordinances against pillaging and public disturbance were issued over and over again.[95] For example, in June 1568, in the interval between the second and third wars, the city issued an order for all residents to arm themselves and report to their guard posts as soon as they received word of an impending disturbance, and special guards were ordered to patrol each *quartier* on holidays. An armed guard accompanied the traditional Corpus Christi Day procession.[96]

Many Parisian Protestants left the city at the outset of the second war and did not return until after the conclusion of the third. They were ordered out of Paris after the abortive coup of Meaux and, before the peace of Longjumeau, were subject to arrest if they returned.[97] Those who were residents of the city and had not previously taken up arms were allowed to remain in Paris during the third war, but they were subject to severe restrictions and were usually required to post a bond to guarantee good behavior. Men whose living depended on daily labor were permitted to go to work, but they were not allowed out on Sundays or holidays. The other Huguenot residents were forbidden to leave their houses at any time.[98]

[90] Grin, "Journal," 46; Haton, *Mémoires*, 2: 526–27; and La Fosse, *Journal d'un curé ligueur*, 93. For the royal orders regarding Paris, see AN, $X^{2b}51$ (30/3/68).

[91] "What does our queen gain from negotiating that peace?" "Hate." Nathanaël Weiss, "Les Protestants parisiens entre 1564 et 1569," *BSHPF*, 50 (1901): 634.

[92] Haton, *Mémoires*, 2: 527–31.

[93] Pasquier, *Oeuvres*, 125–26.

[94] *Reg. BV*, 6: 67.

[95] For a few of the orders regarding public disturbances, see *ibid.*, 6: 76, 82, 87.

[96] *Ibid.*, 6: 38–40.

[97] *Ibid.*, 5: 610, 6: 8–17. For arrest records of persons accused of failing to leave the city when ordered to do so, see AN, $X^{2b}51$ (18/3/68), (20/3/68), (24/3/68).

[98] For orders on public security, see *Reg. BV*, 6: 69–78, esp. 76, 78; and AN, $X^{2a}137$ (11/12/68) and

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These measures were intended to prevent violence on the part of the city's Catholics as well as to eliminate danger from the Protestants themselves. Records of Huguenots arrested during the second war indicate that law officers often intervened to save the Huguenots from crowd violence. Similar attacks occurred during the third war. The most dramatic incident of this sort was the assault on Philippe and Richard de Gastines, who were arrested in January 1569 on the charge of celebrating a Protestant Last Supper in their house on the rue St.-Denis.

Haton may have exaggerated when he said that fifty people were killed in the riot that accompanied the arrests, but other sources confirm that the seizure of the Gastines, along with several of their relatives and neighbors, took place amid widespread public disturbances.[99] Haton reported that this incident was followed by three more collective attacks on Protestants. "The Huguenots were so hated by the Parisian populace that, if the king and authorities had let them have their way, there would not have been one [Huguenot] in the whole city who was not attacked."[100] De Thou added that crowds of people followed after the magistrates of Parlement when they left the Palais de Justice and so threatened them that they eventually pronounced a death sentence against the Gastines for a crime that would ordinarily have warranted banishment or a mere fine.[101]

On July 1, 1569, in accordance with a sentence rendered by Parlement the previous day, Philippe and Richard de Gastines and their in-law Nicolas Croquet were hanged in the place de Grève for taking part in illicit assemblies and the rite of the Last Supper.[102] Their properties were confiscated, as was common with executed criminals, but, in an exceptional measure of reprisal, the house of Philippe de Gastines (in which the assemblies were said to have taken place) was torn down. On its site was erected a monument in the form of a massive pyramid topped by a cross. The monument became an important symbol of the religious hatreds of the Parisian populace.

The people were given another symbol in the summer of 1569 when Admiral Coligny, the leader of the Huguenot party after the death of Condé, was hanged in effigy in the place de Grève. In a move that was at least partly motivated by the desire to confiscate his properties to the profit of a penurious crown, Parlement convicted Coligny of *lèse-majesté* and sentenced him to be dragged on a wooden frame through the streets of Paris to the place de Grève, there to be hanged and strangled. Since Coligny was out of reach of the law when the sentence was pronounced, it was carried out on a straw dummy dressed in his colors and painted to resemble him.[103] Haton recounted that the effigy and Coligny's arms hung in the place de Grève until after the conclusion of peace. He saw them there on several occasions. When rain ruined the dummy's face, it was replaced with a painting of

(21/1/69). For arrest records of accused Huguenots, see AN, $X^{2a}137$, $X^{2b}55$–$X^{2b}62$ *passim*; and AP, $A^{B}3$ *passim*.

[99] Haton, *Mémoires*, 2: 571; and Bordier, "Poursuites et condamnations," 640.

[100] Haton, *Mémoires*, 2: 570–72.

[101] De Thou, *Histoire universelle*, 6: 272–73.

[102] AN, $X^{2b}56$ (30/6/69). La Fosse and Grin accused the Gastines of also giving financial aid to the rebels; La Fosse, *Journal d'un curé ligueur*, 102; and Grin, "Journal," 50.

[103] *Journal de Bruslart*, in Sécousse, *Mémoires de Condé*, 1: 207.

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Coligny that hung from the gallows on a chain and told passers-by of his crimes. What remained of the effigy was taken to the gallows of Montfaucon, where it joined the decaying corpses of executed criminals.[104]

This symbolic execution became reality on St. Bartholomew's Day, when the real corpse of Coligny, the first victim of the massacre, was dragged through the streets, mutilated, and eventually hung by its feet (the head had been cut off) at Montfaucon. Haton added that the youths responsible for these acts conducted a mock trial of the admiral, "just as if they had been judges," and sentenced him to the punishment pronounced by the court three years earlier.[105]

The cross that marked the destruction of Philippe de Gastines's house also became a symbol of Parisian resistance. The Peace of St.-Germain, which ended the third religious war in August 1570, was, like the earlier peace treaties of Amboise and Longjumeau, bitterly opposed by the city's populace. Once again, Parisians protested orders to disarm and allow Protestants to return to their homes.[106] One of the clauses of the edict of pacification specified that all monuments to the persecution of Huguenots were to be demolished and the properties on which the monuments stood returned to their owners.[107] The people of Paris, however, refused to allow the cross to be torn down.

City officers, fearing sedition, ignored the king's orders to tear down the cross through the fall of 1571,[108] but by early December the king's anger was great enough to force the officials to act. In a compromise move intended to appease the populace yet obey the king, city officers made plans to transfer the pyramid to the Cemetery of the Innocents. They explained that the cross was being moved from profane to sacred ground.[109] But Catholic preachers interpreted the action as an attempt to obliterate the memory of Christ's passion, and in their sermons they condemned the removal of the monument.[110]

Because of the high level of tension, preparations for moving the cross had to be made under armed guard. Even so, when the workers retired at night, youths came in and destroyed their labor. On December 8, twenty-five or thirty men were stationed in the cemetery to protect the foundations that had been dug for the

[104] Haton, *Mémoires*, 2: 565–66. Also see La Fosse, *Journal d'un curé ligueur*, 110. Coligny was again hung in effigy after St. Bartholomew's Day. See La Fosse, *Journal d'un curé ligueur*, 155. Mederic Fremyer, a Parisian painter, asked the court for the ninety-six *livres* owed him for the manufacture of two effigies of Coligny, one made of cloth and hung in the place de Grève and one made of wood and hung at Montfaucon, and for fourteen paintings of Coligny's arms, which were dragged through the streets of Paris; AN, $X^{2b}74$ (7/2/73).

[105] Haton, *Mémoires*, 2: 680; Jules Gassot, *Sommaire mémorial (souvenirs)*, ed. P. Champion (Paris, 1934), 105; and Théodore Agrippa d'Aubigné, *Histoire universelle* (Paris, 1886), bk. 6, chap. 4. On the acting out of magisterial roles in religious violence, see Davis, "Rites of Violence," 162–64.

[106] *Reg. BV*, 6: 178–79, 189–90.

[107] For the Peace of St.-Germain, see Stegmann, *Edits des Guerres de Religion*, 77.

[108] *Reg. BV*, 6: 381. For a possible allusion to this decision, see *ibid.*, 6: 363. Also see Haton, *Mémoires*, 2: 632; and La Fosse, *Journal d'un curé ligueur*, 132–33. For the most detailed account of these events, see AN, $X^{2b}1094$. I am grateful to Alfred Soman for this reference and also for the assistance and encouragement he has given me in dealing with the notoriously difficult orthography of the *plumatifs* and interrogations of the Parlement of Paris.

[109] *Reg. BV*, 6: 399.

[110] *Ibid.*, 6: 419 n. Also see La Fosse, *Journal d'un curé ligueur*, 134–39, esp. 134–35; and Simon Vigor, *Sermons catholiques sur les dimanches et festes depuis l'octave de Pasques jusques à l'Advent*, 2 vols. (Paris, 1597), 2: 228–44, esp. 242–44.

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pyramid. Attacked by a crowd that broke down the cemetery's locked gates, they fled under a hail of stones.[111] That night the city called on all of its forces to maintain order and prevent sedition. Householders were ordered to take up arms and station themselves in their doorways, where they were to await further orders. All other arms were to be carefully locked away. Heads of household were made responsible for the actions of their children and servants, and heads of colleges were instructed to keep all of their students inside. The city's professional guards were sent to strategic stations, and the officers of the Châtelet were ordered to patrol the streets of their *quartiers*. Reinforcements were ordered for the *quartier* St.-Denis adjacent to the cemetery. And, finally, Catholic priests were told to admonish their parishioners and warn them against sedition.[112]

The precautions were insufficient. By 5 A.M. on the ninth, a Sunday, crowds had gathered around the houses of known Huguenots. The mistress of the Golden Hammer on the pont Notre-Dame, known to be an in-law of the Gastines, came to the Hôtel de Ville to complain that a great number of people, whom she did not recognize, were throwing rocks at her house and trying to break into it.[113] The city sent troops, who dispersed the crowds, but later in the day the situation worsened. The Golden Hammer and the houses of several other known Huguenots were pillaged, and the guards around the Cemetery of the Innocents were once again stoned and forced to flee.[114]

The vigilance of city officials kept Paris calm for the rest of the week. The following Sunday, people were again ordered to stay in their houses, while the heads of households kept watch. Despite more angry letters from the king, who threatened to send troops to the city to see that his will was enforced, city officials continued to postpone moving the cross.[115]

Finally, late on the night of December 19, the cross was moved. By six the next morning, the crowds had gathered again. Only quick action by the *prévôt des marchands* prevented them from seizing weapons stored in the Hôtel de Ville. The crowds went on, however, to sack the Golden Hammer again and to attack a neighboring house, the Pearl. They then set fire to belongings that had been emptied from both houses and burned a house owned by heirs of the Gastines.[116]

According to Jacques Faye, the house was burned not only because it belonged to the Gastines family but also because the ironsmith who lived there had lent some tools to those responsible for moving the cross. Faye also remarked that the crowds burned the belongings from the Golden Hammer to show that they pillaged not to enrich themselves but to "punish the lady of the house." The crowd believed that she had gone to Coligny and convinced him to pressure the king into tearing down the cross.[117] Other sources confirm the rioters' intention to punish the

[111] *Reg. BV*, 6: 398, 400.
[112] *Ibid.*, 6: 401–02.
[113] *Ibid.*, 6: 406.
[114] *Ibid.*, 6: 406–07.
[115] *Ibid.*, 6: 417–22, 422 n.
[116] *Ibid.*, 6: 423–28.
[117] Jacques Faye and Charles Faye, *Lettres inédites*, ed. Eugène Halphen (Paris, 1880), 32.

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relatives of the Gastines, who, they believed, were responsible, along with Coligny, for the decision to move the cross.[118]

This same specificity of targets shows up in the St. Bartholomew's Day massacre. The inhabitants of the Golden Hammer were among the first victims of the crowd's violence on August 24, as were the inhabitants of the Pearl and other houses attacked in the riot over the cross of the Gastines.[119] Indeed, a great many of the victims can be identified as participants in earlier quarrels or conflicts. Among other names on the lists of the murdered are those of several booksellers previously arrested for the sale of clandestine literature, an innkeeper accused of harboring fugitive Protestants, a *quartenier* removed from office for his religious beliefs, and a maker of rosary beads who had fought for the return of his house, which had been rented to Catholic tenants during the wars.[120] The common people who were victims of the massacres may seem anonymous from a distance of four hundred years, but they were not anonymous to their murderers. They were neighbors, even acquaintances. Their houses were known by the signs of their absence during the troubles or by the regular visits of Châtelet officers. Paris's Protestants were a marked people, a familiar enemy.

Were the murderers and rioters anonymous, if the victims were not? This is more difficult to determine. I have been forced throughout this article to refer amorphously to "the Parisian populace" or "Catholic crowds" in describing the participants in violence. No records exist that would enable me to devise an accurate profile of the groups responsible for the anti-Huguenot riots. The handful of cases I have found in which Catholics were arrested for attacks on Huguenots or for defiance of civil authorities in the period prior to St. Bartholomew's Day involve artisans, day laborers, and one individual who claimed to be a student.[121] These cases would seem to support the statements that frequently appear in both official and unofficial accounts blaming the common people for the violence and dissociating "the better sort" of people from it.[122]

We must not, however, take this dichotomy too literally and assume that Paris was neatly divided between violent common people and law-abiding *gens de bien*. An important part of the population, of whatever social status, hated and feared the religious violence. These people stayed in their houses when the rocks started to fly.[123] Even so, the ability of the authorities in the city to command the obedience of the turbulent fraction of the population was sorely limited.

[118] "Discours touchant la Croix de Gastines," in Cimber and Danjou, *Archives curieuses*, 6: 477–78.

[119] For the attacks on the Le Mercier family (Golden Hammer), see AN, $Z^{1h}71$ (3/9/72) and $Q^{1*}1099^{200}$, ff. 69v-70r (28/2/73). On the Dalencourt family (Pearl), see AN, $Q^{1*}1099^{200}$, f. 64 (15/1/73).

[120] AN, $Z^{1h}71$ (16/9/72), (18/9/72); and "Les victimes de la Saint-Barthélemy à Paris... d'après documents contemporains," *BSHPF*, 9 (1860): 40.

[121] AN, $X^{2a}129$ (16/2/62), $X^{2a}131$ (8/3/63), $X^{2a}133$ (12/29/65), $X^{2b}42$ (14/6/66), $X^{2b}48$ (23/6/67), $X^{2b}49$ (19/8/67).

[122] AN, $X^{2b}1094$ (11/12/71); *Reg. BV*, 6: 416–17 (13/12/71); G. Fagniez, "Mémorial juridique et historique de M^{e} Guillaume Aubert...," *MSHParis*, 36 (1909): 66; and Faye and Faye, *Lettres inédites*, 32.

[123] De Thou, whose *Histoire universelle* is often taken as a firsthand account of St. Bartholomew's Day, in fact wrote in his memoirs that he was so horrified by the two deaths that he saw when he went to mass on August 24, 1572, that he returned home and did not go out again for several days; Jacques-Auguste de Thou, *Mémoires*, ed. Michaud and Poujoulat, ser. 1, vol. 11 (Paris, 1838): 276.

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THE EVENTS OF THE FIRST DECADE of the religious wars show that it was simply not possible for Parisian authorities to prevent riots, nor was it just the religious quarrels that left city officials helpless. In the spring of 1572, there was a rash of attacks on Italians, who were accused of the ritual murders of little children. On June 20, officers of the city guard rescued one Italian from an angry crowd, and three more were found to have been seriously injured. There was no foundation for the accusations. The corpse of one young boy was found, which excited the crowd greatly, but he proved to have died of a long illness and to have been left at the edge of the cemetery by his father, who was too poor to bury him. The hysteria resulted rather from a growing xenophobia, which focused on Italians as partners in the detested fiscal policies of the crown.[124]

The combined forces of the Châtelet, the Hôtel de Ville, and the city guard were not sufficient to maintain order in times of crisis. It was necessary to bring in troops from outside or else call on the city's residents. Both were tricky ventures. Outside troops were always bitterly resented; they might ensure order but did little to diminish tensions. Arming the city's residents and turning them loose in the streets, on the other hand, meant risking chaos. In September 1562 the officers of the Hôtel de Ville boasted that, "if some disturbance took place in a part of the city, the tocsin would be sounded in the parishes, and in one hour 50,000 armed men would be out in the streets of the city ready to use their weapons."[125] What the city officers did not say was that this hastily assembled militia had virtually no training and no discipline. There was little guarantee that these men would await the orders of their superiors rather than rush out to pursue their own ideas of the defense of God and king. The best example of the danger is the disorder that occurred at the outbreak of the second war, when the rumor of a Protestant attack sent thousands of armed men with white crosses on their hats into the streets for several days of uncontrolled violence.

The authorities recognized the danger of arming the Parisians. Every time that peace was made, the king ordered the city to disarm, or at least to reduce its forces, and city officials experimented with different sorts of guard corps and street patrols in order to minimize internal disorder. It is significant that during the riots over the cross of the Gastines, city officers firmly refused the request of residents in the rue St.-Denis to name a chief. They were allowed to arm themselves but were ordered to remain in their houses. City officials feared the consequences of rearming.[126]

Time and again we can see a pattern of tensions building in the city. This is true once more in 1572. The cross of the Gastines riots, which began in December 1571, show that the Parisians were by no means reconciled to the Peace of St.-Germain, and other events indicate that this discontent persisted into the new year. In May

[124] AN, X^{2b}1094 (20/6/72), X^{2b}71 (20/6/72); and *Tocsin contre les massacreurs et auteurs des confusions en France* (Reims, 1572), f. 55. In 1569 the Venetian ambassador, Giovanni Corero, reported that, because of the hated tax program, Italians were so detested that they could not go out in the streets in safety; Tommaseo, *Relations des ambassadeurs vénitiens*, 2: 143.

[125] *Reg. BV*, 5: 137.

[126] *Ibid.*, 6: 413.

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it was necessary for the king to write to the city and Parlement to insist that the peace treaty be strictly enforced.[127] But further tensions were growing on account of the preparations for the marriage of the royal princess Marguerite to the Huguenot prince of Navarre. Here was a new theme for the Catholic preachers, who told their listeners that God would surely be avenged for the impiety of this perverse union.[128] Parisian Catholics were also disturbed by the large number of Huguenots who gathered in and around the capital for the wedding, and they were angered by reports that religious services were being held in Huguenot circles.[129]

Tensions rose to a fever pitch on August 22 when Coligny was wounded by shots fired from the window of a house near his lodgings in the rue de Béthisy. City officers feared a riot and took steps to diffuse the tension. Merchants who had closed their shops because they feared disturbances were ordered to reopen them; people who had taken up arms were ordered to put them down. Only the professional officers of the city's guard were allowed to take up their weapons, and they were cautioned to cause as little stir as possible when they assembled.[130] The precautions were futile. By August 23, the rumor that the Protestants demanded revenge for the attack on Coligny was making the rounds of the city. Once again anger was coupled with fear.[131]

This was the situation in Paris on St. Bartholomew's Eve. We cannot know for certain what Catherine and the king told the *prévôt des marchands* when they summoned him to the Louvre late that night. We cannot know for certain whether they gave him orders for the security of the city, as he later claimed, or whether they planned together the slaughter that actually occurred. We can, however, scarcely avoid the conclusion that anyone in a position of responsibility at that moment would have known, or should have known, that the sound of the tocsin in the night would touch off a massacre.

[127] *Ibid.*, 6: 455; and Catherine de Medici to Christophe de Thou (4/5/72), BN, MS. Dupuy 801.
[128] *Tocsin contre les massacreurs*, ff. 58v-60r, 68v.
[129] The *prévôt des marchands* warned the prince of Navarre at the reception given in his honor at the Hôtel de Ville to keep the peace and oppose those who might wish to threaten it; *Reg. BV*, 6: 469.
[130] *Ibid.*, 7: 10–11.
[131] For a Protestant account of the rumors that were circulating, see "Relation du massacre de la Saint-Barthélemy," in Cimber and Danjou, *Archives curieuses*, 7: 108, 113. For a Catholic account, see Haton, *Mémoires*, 2: 672–73.

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Vagrants, Beggars, and Bandits: Social Origins of Cuban Separatism, 1878–1895

LOUIS A. PÉREZ, JR.

FOR CUBA, THE PEACE OF ZANJÓN, which ended the Ten Years War (1868–78), marked the passing of an age. In the next two decades, social, political, and economic changes unleashed during the separatist struggle transformed the colony. By 1895, when the second rebellion against Spain began, many, like planter attorney Raimundo Cabrera, saw in the new uprising the coming of the Cuban revolution. "Without question," Cabrera wrote to a friend in the United States, "this has not been like the Ten Years War—not in its origins, or in its means, or in its expansion, or much less in its social, political, and economic aspects. Cuba today is revolutionary. . . . Everything is undone and in transition."[1]

In the period between the wars, property relations and production modes were in transition, social formations in flux, commercial ties and political loyalties changing. The socioeconomic order of pre-1878 Cuba broke down, and efforts of Spanish administrators to restore order failed. What developed in Cuba was a type of outlawry similar to what Eric Hobsbawm has described as social banditry—lawlessness as social protest. Social bandits gained support from Cuba's farmers and peasants—many driven from their lands by the planters' expansion of their sugar latifundia in the post-Zanjón era. In particular, insurrection flourished in the rich sugar provinces of western Cuba—once the most faithful of the Ever Faithful Isle. By 1895, peasants across the island had united in a struggle for independence, and, as a result of the social and economic dislocation of the past two decades, a struggle to separate from Spain became a movement to reform Cuban society.

THE TEN YEARS WAR PROFOUNDLY DISRUPTED Cuba's economy. Sugar estates operating before the war on marginal profits—run by planters lacking either the finances or the foresight to modernize their mills—were among the earliest

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[1] Cabrera to José Ignacio Rodríguez, September 18, 1896, Library of Congress, Washington, D.C., Manuscript Division, José Ignacio Rodríguez Papers.

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casualties. Of 41 mills in operation around Sancti-Spíritus in Las Villas province in 1861, only 3 remained after the war; in Trinidad, only 16 of 49; in Santa Clara, 39 of 86; in Cienfuegos, 77 of 107. In Güines, almost two-thirds of the 87 mills operating before the war had disappeared by 1877. In some districts of the eastern provinces, the collapse of sugar production was all but complete. None of the 24 mills in Bayamo and the 18 mills in Manzanillo survived the war; in Holguín, only 4 of 64 mills remained in operation; in the district of Santiago de Cuba, 39 of 100; in Puerto Príncipe, 1 of 100. In all, the total number of mills declined from 2,000 in 1860 to 1,190 in 1877.[2] Sugar production also declined markedly. In 1868, Cubans had produced a record crop of 749,000 tons. Production decreased to 547,000 tons in 1871 and 520,000 tons during the last full year of war. Producers enjoyed a brief recovery in 1879 with a crop of 670,000 tons but suffered successive setbacks in 1880 (530,000 tons) and 1881 (439,000 tons).[3]

The end of the war did not end the crisis. Planters faced new problems. Capital was scarce and credit dear. In post-Zanjón Cuba, high interest rates diminished the ability of local creditors to underwrite economic recovery; rather, the interest rates all but guaranteed that foreign capital would be the principal source of credit for the heavy and long-term investments necessary to revive sugar production.[4]

These developments in Cuba had far-reaching international repercussions. The decline of Cuban sugar exports stimulated the expansion of sugar production worldwide. After Zanjón, therefore, Cuban planters faced greater competition, not only from new producers but also from old competitors. Rival international producers had not had as great an opportunity to extend their share of the world market since the end of the eighteenth century, when revolution in Saint-Domingue ended French supremacy over sugar production. They did not hesitate. In the United States, new varieties of cane were introduced in the South, while beet sugar production expanded in the West. Beginning in 1876 cane sugar from Hawaii entered the United States duty free. Sugar production also expanded in Latin America, most notably in Argentina, Peru, and Mexico. Many displaced Cuban planters resettled in Santo Domingo and helped increase production of Dominican sugar. But it was in Europe that production, in the form of beet sugar, soared. By the mid-1880s, France, Austria, and Germany had become the largest suppliers of sugar for the world market. Production of beet sugar, in 1853 only some 14 percent of the total world production of sugar, by 1884 made up 53 percent of the international supply. Even metropolitan Spain was not immune to the lure of profits from beet sugar. In 1882, two beet factories began operations in Granada and Córdoba; two more opened ten years later in Zaragoza and

[2] José R. Alvarez Díaz *et al.*, *A Study on Cuba* (Coral Gables, Fla., 1965), 91–92; Ramiro Guerra y Sánchez, *Sugar and Society in the Caribbean: An Economic History of Cuban Agriculture* (New Haven, Conn., 1964), 63, and Guerra y Sánchez *et al.*, *Historia de la nación cubana*, 7 (Havana, 1952), 153; H. E. Friedlander, *Historia económica de Cuba* (Havana, 1944), 432; Francisco López Segrera, *Cuba: Capitalismo dependiente y subdesarrollo (1510–1959)* (Havana, 1982), 124–26.

[3] Susan Schroeder, *Cuba: A Handbook of Historical Statistics* (Boston, 1982), 261. Manuel Moreno Fraginals used slightly different figures but recorded similar fluctuations. See Moreno Fraginals, *El ingenio: Complejo económico social cubano del azúcar*, (Havana, 1978), 37.

[4] Alvarez Díaz, *Study on Cuba*, 93.

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Aranjuez. Spanish beet production increased from 35,000 tons in 1883 to 400,000 in 1895.[5]

In addition to new sources of competition and loss of old markets, Cuban planters confronted a precipitous decline in the value of their principal product, even as they prepared to resume production. In 1884 the price of sugar plummeted to a new low, dropping from eleven to eight cents a pound. The drop in sugar prices occurred simultaneously with the transition from slave labor to wage labor and the imposition of a new series of taxes. Heavy taxes assessed against agriculture and livestock, municipal taxes on land, sales taxes, transportation taxes, duties on imported equipment and food—"everything that the people eat comes from abroad," U.S. Vice Consul David Vickers noted—threatened the planter class with extinction. Vickers continued, "Through want of frugality and foresight and with enormous taxation, added to the competition of other sugar countries, the planter, to meet all demands, has discounted his crops at such ruinous rates of interest, piling mortgage upon mortgage, that today he finds himself irrevocably involved in debts equal to at least one year's excellent crop and in some instances much more. In the event of a poor crop, he would not have enough money either to pay current expenses or even to commence grinding his cane when the harvest begins, and no one to loan it to him."[6] Early in 1884 the U.S. consul in Havana reported, "Out of the twelve or thirteen hundred planters on the island, not a dozen are said to be solvent."[7]

Before the war planters had worried about producing large harvests as a hedge against disaster. During the 1880s they produced good crops, but the postwar problems—fewer markets, falling prices, a dwindling labor supply, rising taxes, increased operating costs—forced many into bankruptcy. Within a ten-year period, the U. S. consul in Cienfuegos wrote in 1883, "almost every sugar estate [in] the jurisdiction of Cienfuegos has either changed ownership by reason of debt or is now encumbered with debts to an amount approximating the value of the estates."[8]

By the mid-1880s, Cuba was in a depression. Businesses closed, banks collapsed, and seven of the island's largest trading companies failed. Credit, dear after Zanjón, was almost nonexistent a decade later. In October 1883 the Bank of Santa Catalina closed. In March 1884 the most important savings institution in Havana, the Caja de Ahorros, suspended payments, ostensibly because of the suicide of the bank's president. "It is more probable," the U. S. consul in Havana speculated, "that the Director committed suicide because the bank was unable to meet its

[5] Hugh Thomas, *Cuba, the Pursuit of Freedom* (New York, 1971), 272.

[6] Vickers to Assistant Secretary of State John Davis, October 24, 1883, National Archives, Washington, D.C., General Records of the Department of State, Record Group 59, Despatches from United States Consuls in Matanzas, 1820–1899 (hereafter, Despatches/Matanzas).

[7] Adam Badeau, "Report on the Present Condition of Cuba," February 7, 1884, National Archives, Washington, D.C., General Records of the Department of State, Record Group 59, Despatches from United States Consuls in Havana, 1783–1906 (hereafter, Despatches/Havana).

[8] William P. Pierce to Assistant Secretary of State John Davis, August 10, 1883, National Archives, Washington, D.C., General Records of the Department of State, Record Group 59, Despatches from United States Consuls in Cienfuegos, 1876–1906 (hereafter, Despatches/Cienfuegos).

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engagements."[9] Two weeks later the Caja de Ahorros went into liquidation. In the same month, runs on the Banco Industrial and the Banco de Comercio forced both institutions to close. Two months later, the Banco Industrial went into liquidation. The Bank of Santa Catalina was closely linked to agricultural interests, and its failure principally affected planters. The Caja de Ahorros, on the other hand, served a much broader clientele, including professionals, merchants, shop owners, and workers, and when it failed small depositors of all kinds faced catastrophe.[10] In the first three months of 1884, business failures totaled some $7 million. "The entire population is reduced . . . to blank despondency and universal ruin," the U.S. consul reported.[11]

Similar conditions prevailed in the provinces. In March 1884 the prestigious house of Rodríguez in Sagua la Grande—together with its affiliate in Havana, Miyares and Company—failed. In the once-opulent city of Trinidad, businesses closed, and retail shops were abandoned. The vital rail link to Casilda, the port of Trinidad, ceased operation because of disrepair of the track.[12] A news brief from Santiago de Cuba reported, "Failures, extra-judicial arrangements, and the liquidation of commercial houses follow each other in rapid succession."[13] In Matanzas, the surplus of unsold sugar mounted as prices dropped and markets declined. By 1885 planters could not sell their crops for enough to defray the cost of production.[14] Some three hundred estates faced imminent ruin. "Firms are going into bankruptcy every day," Vickers reported in July 1884. "Planters are discharging their laborers and threaten—to save themselves further disaster—to abandon their estates; gold fluctuates two and three and sometimes ten points a day; all credits are denied even to the most substantial, and men are wondering how and where they will obtain the means to live; and in many cases relatives are doubling up apartment style to save expenses."[15] A month later, according to Vickers, conditions had deteriorated further. "Every day the situation is becoming more and more serious and the condition of the people more and more sad. All credits are being suspended, labors are unpaid, plantations being abandoned. . . . House owners are receiving little or no rent. In a word the condition of all classes, rich and poor alike, is most lamentable; and what is worse, there is not hope in the future."[16]

Foreign travelers confirmed this generally bleak picture of late nineteenth-century Cuba. Maturin M. Ballou, traveling across the island east to west, encountered dislocation and despair everywhere. In Santiago de Cuba, Ballou found the local gas monopoly "on the verge of bankruptcy, like nearly everything else of a business character in Cuba." In Cienfuegos, one planter told Ballou that

[9] Adam Badeau to secretary of state, March 6, 1884, Despatches/Havana.
[10] Gaston Descamps, *La crisis azucarera y la Isla de Cuba* (Havana, 1885), 143.
[11] Adam Badeau, "Report on the Present Condition of Cuba," February 7, 1884, Despatches/Havana.
[12] *El País*, November 26, 1889.
[13] *New York Times*, July 17, 1884.
[14] *Diario de Matanzas*, January 18, 1885.
[15] Vickers to Davis, July 2, 1884, Despatches/Matanzas.
[16] Vickers to Davis, August 27, 1884, Despatches/Matanzas.

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he would use his molasses as fertilizer on cane fields rather "than send it to a distant market and receive only what it cost." The planter vowed to permit "thousands of acres of sugar cane to rot in the fields this season as it would cost more to cut, grind, pack, and send it to market than could be realized for the manufactured articles." "Mercantile credit may be said to be dead," Ballou noted on his arrival in Havana, "and business is nearly at a standstill." As he prepared to leave Cuba he wrote, "Financial ruin stares all in the face."[17]

Government revenues also diminished during the depression. As a result, public services declined, sanitation services decreased, public works programs were suspended, and public administration deteriorated, not least because administrators were without pay. "Employees of the Government and municipalities have received no pay for months," wrote Vickers from Matanzas in 1884. "As a sample, this city is in arrears to the Gas Company over $95,000—to the schools about eighteen months, the police nine months. Even the public hospital—which collects a tax of $2—has been obliged to beg bread from the *bodegas* [grocery stores], and from door step to door step."[18] In Havana the press reported that public officials were obliged to pawn their furniture in one last desperate effort to stave off destitution.[19] The city also had a staggering utility bill of $400,000—and a threat from an impatient Spanish-American Light and Power Company in New York to suspend gas service for city street lights unless the debt was speedily and satisfactorily settled.[20] Ballou reported:

> The streets, even about the paseos [promenades], are so impregnated with filth, here and there, as to be sickening to the senses of the passer-by. Once in 3 or 4 weeks somebody is awakened to the exigency of the situation, and a gang of men is put to work to cleanse the principal thoroughfares. . . . We were told that the reason for this neglect was that no one was regularly paid for work; even the police had not received any pay for seven months; and many refused to serve longer. The soldiery had not been paid their small stipend for nearly a year.[21]

During the 1880s the unemployment rate soared. Jobs were few and competition fierce. In Havana some twenty thousand workers were unemployed. In 1885 the once-thriving Havana naval yard closed, leaving hundreds without work. The decline of cigar exports in the late 1880s and early 1890s wrought havoc on one of the major labor intensive sectors of the Cuban economy. Repercussions were immediate and far-reaching. Cigar production had provided employment for more than one hundred thousand people in agriculture and manufacturing, the vast majority of whom resided in the two western provinces of Pinar del Río and Havana. The cigar factories alone employed some fifty thousand workers. As cigar

[17] Ballou, *Due South; or, Cuba Past and Present* (Boston, 1886), 39, 43, 45, 49, 51–52, 168.
[18] Vickers to Davis, August 27, 1884, Despatches/Matanzas.
[19] *La Lucha*, March 18, 1889.
[20] See Ramon O. Williams to James D. Porter, January 12, 1887, Despatches/Havana; and F. Moreno, *Cuba y su gente (apuntes para la historia)* (Madrid, 1887), 158–59.
[21] Ballou, *Due South*, 133.

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exports decreased, the number of factory closings increased. By the early 1890s, thirty-five thousand cigar makers were unemployed, and the remainder were reduced to part-time.[22] Moreover, during these years, the abolition of slavery was completed, and thousands of former slaves joined the Cuban labor force as free wage earners.

Already dismal employment prospects for Cubans were exacerbated by the Spaniards' continued monopolization of public office. These were hard times in Spain, too, and, from political necessity, imperial ministers viewed public administration in Cuba as a source of relief for unemployed Spaniards. Cuba late in the nineteenth century remained very much what it had been early in the sixteenth century: a place for the destitute of Spain to start over. Public offices and political appointments in the colony were considered both a perquisite of empire and an overseas extension of the patronage system in Spain—from which Cubans were by and large excluded.

But *peninsulares* monopolized more than public positions. They also owned most private property, controlled trade and commerce, presided over banking and finance as well as industry and manufacturing. Spaniards owned the factories and farms, managed the plants and plantations, were the retail shopkeepers and wholesale merchants as well as the moneylenders and land brokers. Spaniards also worked in the professions, in the trades as artisans and apprentices, in the offices as clerks, and in the fields as day laborers. Most of all they controlled the jobs, and, whether by formal contract or by informal consensus, Spaniards preferred to hire Spaniards, a private practice that coincided with public policy. Spain actively encouraged immigration to Cuba as a comparatively convenient and cost-effective method to reduce the size of a socially unstable population at home and increase the number of loyalists in a politically unreliable population in Cuba.

Beginning in 1886, Madrid formally adopted a policy of subsidizing travel costs for all Spaniards seeking employment in Cuba. And they arrived in shipload after shipload. In the decades after Zanjón, one-quarter million emigrated to the island.[23] Not only did more Spaniards come but they also differed from earlier arrivals. The new immigrants were from the north, mostly from Galicia and Asturias. Destitute, often desperate, strong-willed and self-possessed, and, most of all, determined to make it, they worked hard and long, often for meager wages. It was a labor market in which many Cubans could not compete.

In the decades after the Peace of Zanjón, there seemed to be no place for Cubans in Cuba. For many members of the planter class, peasantry, professions, and proletariat, Spanish administration revealed itself incapable of discharging the central clause of the colonial social contract: the opportunity for livelihood. Cubans seemed in danger of becoming a superfluous population, unemployable and expendable, outsiders and outcasts in the society they claimed as their own. They

[22] See *Diario de la Marina*, August 16, 1892; and *El País*, August 24, 1892.

[23] Rafael María Merchán, *Cuba, justificación de sus guerras de independencia* (1896; reprint edn., Havana, 1961), 38; Duvon C. Corbitt, "Immigration in Cuba," *Hispanic American Historical Review*, 22 (1942): 302–08.

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faced all at once exclusion, expulsion, and expatriation. Indeed, emigration was symptomatic of the crisis in colonial Cuba in the late nineteenth century. Thousands of working-class families emigrated to Key West, Tampa, Ocala, and Jacksonville. Some ten thousand cigar workers found employment in the expanding cigar industries of Florida during the 1880s and early 1890s, creating in exile a flourishing community in which Cuban emigres found a livelihood.[24] During the last third of the nineteenth century, approximately one hundred thousand Cubans, in all occupations and professions, of all ages, from all classes and races, emigrated to Europe, the United States, and Latin America.[25]

IN RURAL CUBA THE CRISIS of the 1880s found its most dramatic expression. A new stage of capitalist organization transformed sugar production and with it all of Cuba. Sugar mills continued to go bankrupt after the war, and producers recognized that reorganization of the production system and restructuring of property relations were necessary to regain Cuba's premier position in sugar exports. Their response was swift and the effects sweeping. Not since the first third of the nineteenth century had Cuban sugar producers expanded so aggressively and with such speed as they did in the decade after Zanjón.

The incentive for greater concentration of land originated with the Ten Years War. Property destroyed in war was property discounted in peace. Farms vacated by the forced relocation of rural families, as well as the property of owners killed in the war, provided additional opportunities for alert and ambitious landowners to expand their holdings. Many farms abandoned or destroyed were simply seized. Municipal records were also destroyed and titles lost, and inevitably land claims became confused and contested. Then, too, the property of many Cubans who had joined the separatist cause was expropriated and subsequently auctioned as a means of financing the war.

During the war years expansion was haphazard and fortuitous, sporadic and scattered, more a response to opportunity than the result of organization. But, during the 1880s, planters undertook far-reaching changes—mobilized into action by international developments. Greater efficiency was needed to market sugar profitably under the existing conditions of international competition and falling prices. Production strategies shifted from increasing the number of sugar mills to increasing the production capacities of those already in operation. New credit, fresh capital, and new ownership, principally from the United States, provided larger enterprises with the resources necessary to expand.

Improved varieties of cane, innovations in manufacturing techniques, and other technological and industrial advances became generally available to planters by the

[24] Eusebio Hernández y Pérez, *El período revolucionario de 1879 a 1895* (Havana, 1914), 57.

[25] Fernando Portuondo del Prado, *Historia de Cuba* (6th edn., Havana, 1965), 434. Also see José Rivero Muñiz, "Los cubanos en Tampa," *Revista Bimestre Cubana*, 74 (1958): 5–140; and José Luciano Franco, "Panamá: Refugio de la rebeldía cubana en el siglo XIX," *Casa de las Américas*, 15 (1974): 16–26.

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late 1880s and provided Cuban producers an opportunity to respond aggressively to new conditions. New machinery to extract maximum sugar from cane and efficiently grind the increased volume of harvested cane was introduced. New vacuum pans, boilers, and centrifugal equipment were installed to distill and crystallize more sugar from improved strains of cane. By the late 1870s the Bessemer steel process had reduced substantially the cost of railway construction, and the expansion of rail facilities relieved the bottleneck created when increased production overloaded the transportation system. The two-wheeled wooden carts drawn sluggishly by a yoke of oxen (*carreta*) over dirt roads, many in wretched condition, could not have accommodated the growing needs of expanded sugar production. The new rail system promised to increase efficiency of operations and reduce costs at a time when Cuban planters were in dire need of economy of production. The railroad also served to widen the export market by linking hitherto isolated sugar regions with ports. It encouraged, lastly, the expansion of sugar cultivation and hence increased the land devoted to sugar cane. Producers acquired rapid and reliable access to new lands—this at the expense, however, of smaller farmers.[26]

The shift in production strategies led immediately to a sharper division between field and factory. These developments made growth and specialization not only attractive but also imperative—a condition of survival. Planters unable to meet the growing capital requirements of sugar manufacturing abandoned the industrial end of production altogether and devoted themselves exclusively to growing cane. The system in which the grower milled his own cane gave way to a new specialization of operations and, increasingly, separation of ownership in which the mill owner concentrated on manufacture and the planter tended to cultivation.

Modernization also led to new and greatly enhanced production capabilities of Cuban mills. Planters more than doubled their output in the decade between 1883 (460,000 tons) and 1892 (976,000 tons). Two years later, Cuban planters harvested a crop of one million tons. These advances placed additional pressures on those sectors, including transportation and storage, that supported production. Not only railroad but also wharf and pier construction expanded, and warehouse facilities increased. But, most important, more land was essential.[27] If the newly renovated

[26] Fe Iglesias, "Algunos aspectos de la distribución de la tierra en 1899," *Santiago*, 40 (1980): 152–54; and Félix Goizueta-Mimó, "Effects of Sugar Monoculture upon Colonial Cuba" (Ph.D. dissertation, University of Pennsylvania, 1971), 91–98.

[27] The sugar latifundia expanded at the expense of all other agricultural units, sugar and nonsugar alike, throughout the western regions. In the region of Remedios in Las Villas province, the new regimen of land concentration swiftly established the preeminence of the sugar latifundia. In Yaguajay, the new mill Narciso (1891) absorbed older and less efficient mills, including Soberano, Oceano, Encarnación, Aurora, Urbaza, and Luisiana. In the rich zones of Matanzas-Cárdenas-Colón, sugar planters expanded at a frenetic pace. The revolution in land ownership during the 1880s also transformed the region around Sagua la Grande. Technological improvements and cultivation of new land increased the production of Sagua's six major mills by 50 percent. Nowhere, however, did land concentration occur as suddenly or as spectacularly as in the Cienfuegos region. Between 1884 and 1891, thirteen new mills were organized. These new mills, destined to dominate Cuban sugar production for the next quarter-century, included Constancia, Soledad, San Lino, San Agustín, Lequeito, Caracas, Hormiguero, Parque Alto, and Cieneguita. See Guerra y Sánchez *et al.*, *Historia de la nación cubana*, 192–94; and Friedlander, *Historia económica de Cuba*, 436–38.

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mills were to operate at optimum efficiency and a maximum level of production, they would require much greater quantities of cane, which was possible only by expanding the zones of sugar cultivation. Hence, the first requirement for planters was acquisition of more land; second, cheap labor. The abolition of slavery in the 1880s forced planters to find workers. And, as the scope of sugar production increased, so did the planters' need for a reliable supply of cheap labor.

Sugar planters attempted to secure new land and cheap labor at the same time. First, they had to eliminate rival agricultural enterprises and competing land tenure systems and appropriate units incompatible with sugar production. Immediately, therefore, planters wanted land devoted to tobacco, coffee, fruit and vegetables, and grazing converted to cane and attached to the sugar latifundia. They also wanted the abolition of diverse forms of land ownership. These conditions invited the privileged and powerful to move against the poor and powerless, and circumstances in the decade after Zanjón made these opportunities irresistible. Family farms, commercial estates, and livestock pastures were swallowed up. Unincorporated land was absorbed. But, most important, the *hacienda comunera*, the system of communal farming in which ownership of land was distributed in allotments and held in common by communities in the form of shares (*pesos de posesión*), was threatened. The *hacienda comunera* was one of the principal forms of traditional land tenure in Cuba, consisting of a tract of land owned jointly by any number of individuals and families, the value of which was assessed and ownership allocated in the form of *pesos de posesión*. Tenure of *posesión* guaranteed an owner (*comunero*) title to a certain portion of the tract but was not a clear title to any specific part. *Pesos de posesión* granted a right to land within the larger boundary of the original grant (*merced*). When an owner died, the assessed value of the land was allocated among heirs. The original land grant remained intact, but the right to land was divided in direct proportion to the fixed dollar share. The number of shares in any given tract increased with the passage of time, as each successive generation willed its shares to new heirs or sold its portions to new owners. Rights to the land multiplied in the course of time, and ownership became hopelessly confused and continuously contested. *Pesos de posesión* were willed, sold, and otherwise transferred without regard to the proportion of land corresponding to each share.[28] A special commission examining the surviving *haciendas comuneras* in 1901 reported:

> In so far as the real nature, the location and the area, and the boundary thereof are concerned, there prevails the greatest confusion which can be imagined. The actual title-deeds of said properties are scarce, doubtful, and even unknown. Neither the number nor the real names, nor the exact location, nor the area of the greater part of said undivided properties are known. In many instances, not even those who name themselves tenants in common know if they are really such tenants, nor why they are called tenants in common.[29]

[28] For a general discussion of the *hacienda comunera*, see Francisco Pérez de la Riva, *Origen y régimen de la propiedad territorial en Cuba* (Havana, 1946), 61–66; and Leopoldo Cancio, "Haciendas comuneras," *Cuba y América*, January 1902, pp. 227–36.

[29] Carlos Párraga and Domingo Méndez Capote to Rafael R. Govín, December 26, 1901, National Archives, Washington, D.C., Records of the Military Government of Cuba, Record Group 140 (hereafter, MGC/RG 140), file 4167–1901.

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Land grants were rarely surveyed, and inevitably boundaries were inexact, unofficial, and almost always unprovable. It was precisely this ambiguity that made the *hacienda comunera* wholly incompatible with the requirements of sugar producers. Because ownership of any tract of land was claimed by large numbers of people holding *pesos de posesión*, transfer of the entire unit was all but impossible. The only recourse was litigation to challenge ownership, and for the vast numbers whose titles were imperfect, incomplete, or otherwise flawed the adjudication process was the first step toward displacement. Valid titles, too, were often challenged, and not infrequently the judicial survey (*deslinde*) so reduced the size of a legal claim as to make subsistence agriculture nearly impossible. Court proceedings tended to favor large planters, who could retain talented attorneys, and bribery and corruption often had as much to do with a final judgment as the legal merits of a case.[30] Holders of invalidated claims were required to abandon the land, taking with them livestock, crops, and personal property located within the boundaries of the tract. Immovable property, specifically gardens, planted fields, and orchards, was to be appraised and the owner adequately indemnified.[31]

However vague and imprecise, *pesos de posesión* represented a legal right to the land. For a comparatively modest sum of money, farmers obtained *posesión* and secured the attending right to work some part of the tract. This was *minifundista* farming on a vast scale, a source of subsistence and livelihood for generations of small farmers without the requirement of a large outlay of capital. By the end of the 1880s, this form of land ownership was well on its way to extinction in western Cuba.

Capitalization of agriculture required not only transformation of the existing system of land tenure but also elimination of the peasant as an independent farmer. Persistence of traditional forms of land tenure and the continuance of marginal agricultural production obstructed both monopolization of land and consolidation of the latifundia. But control of ownership through concentration of land gave planters more than control of land use. Monopolization was also a means of coercion to obtain control over the workers of land. Farmers and peasants able to provide for their own subsistence were not inclined to work for depressed wages. The purpose of monopolization was not so much to use land as to withdraw its use, to transform the self-sufficient farmer into a wageworker. The unreliability of the local labor supply was an important motivation for territorial expansion by planters, who often appropriated land they could not and did not intend to cultivate. The effect was to compress peasants onto land incapable of producing enough for subsistence; peasants were then obliged to work on the sugar latifundia.

The expansion of sugar production thus had two interrelated objectives: first, the expropriation of peasant lands to increase the output of sugar and, second, the appropriation of peasant labor to decrease the cost of production. Planters were

[30] Lowry Nelson, *Rural Cuba* (Minneapolis, 1950), 96–97. Also see Rogelio de Armas y Herrara, *Estudio sobre deslindes* (Baracoa, Cuba, 1913); and Antero Regalado, *Las luchas campesinas en Cuba* (Havana, 1979), 47–48.

[31] "Haciendas comuneras," MGC/RG 140, file 4167–1901.

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successful on both counts. Land and labor served production, and the increased need of the latter was a function of the increment of the former.

The capitalization of agriculture and the proletarianization of peasants transformed the character of the rural political economy. Commercial farms, communal lands, and family farms passed under the control of the latifundia. These developments affected all regions of Cuba in varying degrees, but the effects were most pronounced in the sugar zones of the west. Through the late 1880s and early 1890s, tens of thousands of Cubans, white and black, appeared in the census records as landless agriculturalists, working on the sugar estates in Matanzas and Santa Clara.[32]

THE DISLOCATION OF THE POPULATION IN RURAL CUBA had begun with the Ten Years War. The separatist conflict caused havoc in the countryside, forcing thousands of families to migrate to the cities in search of sanctuary and security. Spanish military policy, which forcibly removed some families to urban reconcentration centers, exacerbated dislocation in the countryside and congestion in the cities. Rural refugees became urban indigents. Dislodged from their homes and dispossessed of their property, the displaced families of the war were deprived of a livelihood in peace. The result was immediate pauperization.

The end of the war did not end the crisis. The modernization of mills and expansion of sugar estates meant that displacement of the rural population continued.[33] To the swollen ranks of those who had no reason to leave the cities after the war were added those who had no reason to remain in the countryside after the peace. More rural families migrated to the cities but found their conditions materially worse than the ones from which they sought escape. Unemployed workers, displaced farmers, and emancipated slaves competed with Spanish immigrants for a decreasing number of jobs.

By the end of the 1880s, vagrancy and mendicancy reached unmanageable proportions. For many of the destitute, mendicancy was the only recourse. For

[32] Rebecca J. Scott, "Class Relations in Sugar and Political Mobilization in Cuba, 1868–1899," *Cuban Studies/Estudios Cubanos*, 15 (1985): 21. These developments in late nineteenth-century Cuba were similar to and simultaneous with events occurring elsewhere in the world. "In disparate parts of the world ruled by different states and empires," wrote Joel S. Migdal, "small freeholding peasants almost simultaneously faced catastrophic changes in the rules of land tenure in their societies. It is most striking that these land tenure changes came in a number of countries almost at the same moment in history. . . . Wherever enacted . . . one prime purpose of such laws was to facilitate changes in agricultural production that would increase yields and that would lead to the planting of crops suitable for export. Simultaneous changes in land tenure in seemingly unrelated parts of the globe came in large part because of increased demand in Europe and the United States for cotton, sugar, coffee, jute, indigo, and a number of other select crops. . . . New production techniques for specific crops that were developed at this time also increased the pressure for consolidating land so as to gain greater economies of scale. . . . Whatever the precise purposes and rationalizations for changes in land tenure, they precipitated eruptive, universal dislocations whenever enacted. They signaled changes in agricultural production and class relations that entered so deeply into the fabric of societies that their effects are often still discernible today." Migdal, "Capitalist Penetration in the Nineteenth Century: Creating Conditions for New Patterns of Social Control," in Robert P. Weller and Scott E. Guggenheim, eds., *Power and Protest in the Countryside* (Durham, N.C., 1982), 60–61.

[33] Guerra y Sánchez, *Sugar and Society in the Caribbean*, 85–93.

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some, the threshold between marginality and criminality was easy to cross. Entire sectors of the urban population passed from chronic poverty to permanent indigence. Illness and disease spread through the destitute in Havana. Mortality rates suggest a grim and unremitting life and death struggle in the capital throughout the latter part of the nineteenth century. Epidemics struck again and again: yellow fever, cholera, smallpox, typhoid fever, dysentery, and malaria. Asiatic cholera struck in 1868, raising the annual death rate in Havana to a staggering 51.75 per 100,000 inhabitants. Between 1873 and 1879 and again in 1895, a yellow fever epidemic occurred; in 1871–72, 1874–75, 1878–81, 1887–88, 1891, and 1894, smallpox. Pulmonary tuberculosis claimed some 1,200 lives in 1886. Between 1881 and 1890, the Havana death rate averaged 34.18 per 100,000 inhabitants.[34]

Destitute men and women tended to drift aimlessly from job to job: the women from domestic service to peddling to prostitution, the men from day labor to unemployment to crime. Many lived by their wits, through petty pilfering and swindling but mostly through begging and stealing. By 1885 there were some two hundred registered brothels in Havana. Beggars became a permanent feature of the urban landscape, and robberies, assaults, and petty theft of all kinds spread across the island.[35] "Personal security in Cuba is a myth!" one Spanish traveler complained in 1887. "No one there who leaves his house knows if he will be able to return alive and well. Robbery occurs as much on the most central streets of Havana, at any time of day or night, as in unpopulated areas."[36] Wrote a Cuban in the same year: "In the cities we have the rapacious land sharks, the pickpockets, the snatchers of watch chains—the terrifying cry of 'your money or your life,' assailing the ear of the defenseless passer-by at every obscure corner; the stabbings, the shootings, the cries, the incessant alarms, the rushing to and fro—everywhere a bloody picture of shameful and abominable barbarities."[37] An American correspondent exclaimed: "Havana has more police to the square foot than any other city on earth. People are often waylaid and robbed in the streets, and there is much wailing in Havana . . . because with all this great protection the thieving cannot be stopped."[38]

Few who traveled to Cuba during these years could fail to notice conditions in the cities; what they noticed most were the beggars. Visiting Cienfuegos in the mid-1880s, Ballou wrote:

> It was very pitiful to behold the army of beggars in so small a city, but begging is synonymous with the Spanish name. Here the maimed, halt, and blind meet one at every turn. Saturday is the harvest day for beggars in the Cuban cities, on which occasion they go about by scores

[34] *Census of the Republic of Cuba, 1919* (Havana, 1919), 258. In Europe, mortality rates had not been this high since the end of the seventeenth century. See Fernand Braudel, *Capitalism and Material Life, 1400–1800* (New York, 1969), 62–63.

[35] Raimundo Cabrera, *Cuba and the Cubans* (1887; reprint edn., Philadelphia, 1896), 141. See Benjamin de Céspedes, *La prostitución en la ciudad de La Habana* (Havana, 1888); and Ignacio D. Ituarte, *Crímenes y criminales de La Habana* (Havana, 1893).

[36] Moreno, *Cuba y su gente*, 179–180.

[37] Cabrera, *Cuba and the Cubans*, 236.

[38] *New York Times*, March 12, 1893.

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from door to door, carrying a large canvas bag. Each family and shop is supplied with a quantity of small rolls of bread, specially baked for the purpose, and one of which is nearly always given to the applicant on that day, so the mendicant's bag becomes full of rolls. These, mixed with vegetables, bits of fish, and sometimes meat and bones when they can be procured, are boiled into a soup, thus keeping soul and body together in the poor creatures during the week.[39]

"Numerous mendicants," wrote one American journalist, "are daily seen in nearly all the principal streets, clothed in filthy rags, and soliciting alms with much impunity."[40] The U.S. consul in Havana wrote often of the "numerous vagrants infesting this city," attributing the widespread "vagrancy and kindred social evils" to the "scarcity of employment for its people."[41] English historian John Anthony Froude, visiting Havana during the mid-1880s, described legions of beggars crowding the capital's narrow streets. Squalor and destitution were everywhere.[42] Some years later American tourist Richard Davey struck a similar tone: "Never . . . have I seen such terrible beggars as those of Cuba. They haunt you everywhere, gather round the church doors, whining for alms, insulting you if you refuse them and pestering you as you go home at night, never leaving you till you either bestow money on them or escape within your own or some friendly door."[43]

Not all of these problems were new. Vagrancy had caused authorities concern during the Ten Years War. Their expectation that the end of the war would relieve the urban congestion and destitution, however, proved short-lived. Within a year after Zanjón, Spanish authorities undertook a thorough policy review of the relationship between vagrancy and rising incidents of lawlessness. It was a connection readily established, if not easily ended. What initially preoccupied officials was a search for a remedy for the causes of vagrancy as a means of reducing its effects. Attributing vagrancy to idleness and unemployment, colonial authorities adopted three courses of action, each designed to meet problems associated with a specific age group among vagrants. First, vagrant youths under twenty years of age were to be assigned to local factories and farms as a way to promote a work habit and provide them with a trade. In a second program, the government established a workhouse for beggars and vagrants on the Isle of Pines. In the *protectorado del trabajo*, as the Isle of Pines project became known, vagrants between the ages of twenty and fifty were to be sentenced to work rehabilitation programs with minimum compensation. Third, vagrants over the age of fifty were to be assigned to local town councils (*ayuntamientos*) as day laborers in public works and government service with compensation adequate for their upkeep.[44]

In the best of times, these would have been ambitious programs. In post-Zanjón Cuba, they were impossible. The *protectorado del trabajo* was hardly launched when

[39] Ballou, *Due South*, 49.
[40] *New York Times*, December 19, 1890.
[41] Ramon O. Williams to George L. Rives, November 24, 1888, Despatches/Havana.
[42] Froude, *The English in the West Indies* (London, 1888), 301–06.
[43] Davey, *Cuba, Past and Present* (New York, 1898), 137.
[44] "Memoria sobre la represión del bandolerismo y destino de los vagos," December 16, 1879, Archivo Nacional de Cuba, Havana (hereafter, ANC), Fondo Gobierno General, file 584, no. 28863.

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revenue deficits forced its suspension. By the early 1880s it was no longer feasible to condemn the idle to work when workers were condemned to idleness. Nor could municipal government bear the rising cost of subsidizing antivagrancy programs at a time of declining revenues and deepening indebtedness. Public payrolls were already hopelessly in arrears, which all but foreclosed new municipal expenditures.

Through the 1880s, authorities resorted to a variety of measures to end vagrancy, all of which had one feature in common—failure. As early as 1881, the civil governor of Santa Clara organized the Junta de Represión de la Vagancia designed to halt the rising crime rate through the systematic and summary arrest of all vagrants.[45] In 1888, deteriorating conditions in Havana led to the promulgation of severe measures. "The disagreeable spectacle offered by the great number of individuals of all classes who are continually found asleep or given over to idleness, in the promenades and other public places, when not to worse acts," the civil governor of Havana, Carlos Rodríguez Batista, proclaimed, "must not and cannot be tolerated any longer." "The measures adopted by this Civil Government," he continued, "on different occasions for the disappearance of such scenes were intended also to guarantee individual security, particularly at night, when people on foot are easily surprised and attacked by criminals under the disguise of peaceable persons, thus originating the commission of crimes that were it not for those circumstances would be easy for the police to prevent." Rodríguez Batista decreed the arrest of "every person without occupation or known address found in the parks, streets, and other public places of the city." Vagrants were sentenced to mandatory employment in public works projects in Havana, without compensation.[46] At the same time, the colonial government sought to revive the *protectorado del trabajo*, this time imposing a tax levy on each province to subsidize the program.[47]

These measures had little immediate effect. Within months, a new governor complained of rising incidents of crime and no decrease in vagrancy. In 1889, Governor Manuel Salamanca pledged his administration to create new jobs as an antidote to vagrancy, "the single largest cause of crime."[48] But Spanish authorities knew that colonial revenues were not adequate either to promote public works projects or to create jobs. Nor could officials expect relief from private quarters, for the Cuban economy continued in depression. The colonial authorities devised still another plan—the expulsion of vagrants and criminals. Beginning in the late 1880s, and continuing through the early 1890s, Spanish authorities offered moral encouragement and material incentives to "undesirable" Cubans to emigrate to Florida. Convicts received the choice of prison on the Isle of Pines or passage to

[45] Emilio Calleja, Santa Clara, to governor general, January 27, 1881, ANC, Fondo Gobierno General, file 584, no. 28863.

[46] *Diario de la Marina*, November 24, 1888.

[47] Antonio Pérez, secretary, Audiencia of Havana, to administration section chief, January 9, 1888, ANC, Fondo de Asuntos Políticos, file 81, no. 23. Also see Gobierno de la Isla de Cuba, "Circular: A los Gobernadores Civiles de las Provincias de esta Isla," January 19, 1888, ANC, Fondo de Asuntos Políticos, file 81, no. 23.

[48] Salamanca, "Circular," May 12, 1889, ANC, Fondo Gobierno General, file 584, no. 28863.

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Key West. Fugitives at large were offered pardons on the condition that they surrender to authorities preliminary to their emigration. A new population of Cubans thus joined the growing expatriate communities in Florida, and, although it is impossible to determine the number of Cubans emigrating to Florida under these circumstances, the figure was large enough to prompt the major of Key West in 1888 to protest Spanish policy to the State Department.[49]

NOT ALL FARMERS MIGRATED TO THE CITIES. And not all who remained in the countryside either acquiesced in their impoverishment or accepted the new order. Some were prepared to endure grinding poverty but few the collapse of the world as they knew it. Discontent increased, slowly at first, and resentment mounted. Within a decade after Zanjón, banditry had reached epidemic proportions in the countryside, a palpable expression of the crisis in rural Cuba. Many displaced farmers and dispossessed peasants refused to submit to the regimen of the plantation and enter the service of the planters, choosing independence and life outside the law. But banditry was more than a condition of individual rebellion and personal revolt. It was also an expression of collective resistance by communities threatened with the loss of a traditional way of life.

The increase in lawlessness during the 1880s was in part a result of the insurgency of the 1870s. The Ten Years War had disrupted the lives of thousands of Cubans, and for veterans and noncombatants (*pacíficos*) alike the requirements of peace were often more exacting than the rigors of war. Peace gave many Cubans little incentive to abandon arms. Jobs were few, agriculture in ruins. Even when farms could be reclaimed, the destruction of crops, the dispersal of livestock, and the disrepair of equipment were so complete and the cost of beginning anew so great that all but the most determined were discouraged from returning to the land. Many had no place to return to or did not want to return, and for them it was preferable to seek survival as unrepentant fugitives than to scratch out a living as indigent farmers.[50]

But the Ten Years War as the effective explanation for increased lawlessness is neither sufficient nor satisfactory. The war was fought largely in eastern Cuba. The eastern population had suffered most from the military campaigns, and there the war had been most destructive and disruptive. But nothing comparable to the conditions of lawlessness in the west occurred in the east. Postwar banditry was not unknown in Camagüey and Oriente, but in the course of a thirty-month period, between May 1885 and November 1887, the governor of Camagüey reported no instance of banditry. Not until June 1888 did authorities in Camagüey record the first occurrence. In Oriente, acts of lawlessness were confined to scattered cases of arson and sporadic instances of assault.[51]

[49] Ramon O. Williams to George L. Rives, January 28, 1888, Despatches/Havana. Also see Williams to Rives, February 3, 1888, Despatches/Havana.

[50] Portuondo del Prado, *Historia de Cuba*, 502.

[51] "Expediente promovido sobre las proporciones que van tomando el bandolerismo de Puerto Príncipe," March 31, 1887, ANC, Fondo de Asuntos Políticos, file 81, no. 15; and "Expediente

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The disruption produced by the separatist war, hence, does not alone explain the increase of lawlessness in the western provinces. A more direct and relevant correlation is to be found in the different patterns of land tenure and tenancy between the two regions of the island. Vast areas of eastern Cuba remained uninhabited and unchartered. More land was available to more people in Oriente than in the west, and the availability of land mitigated the effects of postwar disarray and destitution. Access to land provided individuals with subsistence and communities with sustenance, which eased the transition from war to peace.

Long after communities of small farmers had declined in the west, communal ownership and individual farms continued to flourish in the east. Whereas the sugar latifundia emerged as the principal agricultural form in the west, mixed small farms continued as the prevalent form of land tenure in the east. Even as late as 1899, Oriente contained the largest number of independent farms with the smallest average acreage. Only 0.5 percent of the farms in Oriente were over 330 acres, and they comprised only 26.9 percent of the total area under cultivation. The average size of the 21,550 farms in Oriente in 1899 was approximately 83 acres. Oriente claimed not only the highest number of individual landowners but also the highest number of renters, a total of 43,721. The contrast with the western sugar regions in 1899 was remarkable. Matanzas had the fewest number of farms—4,083—with the largest average acreage—247; followed by Havana, 6,159 farms, each averaging 135 acres; and Santa Clara, 16,129 farms averaging 115 acres.[52]

Peace, not war, disrupted community organization and property relations in the west. Not only did consolidation of the sugar latifundia displace farmers from the land and transform peasants into a rural proletariat but capitalization of agriculture also created a labor force for which there existed neither permanent employment nor steady work. Employment was seasonal, and the length of the season was always subject to the vagaries of the world market. During the harvest (*zafra*), typically between late autumn and early spring of every year, work was available and often plentiful. The aptly named "dead season" (*tiempo muerto*), occurring during the tropical summer, followed the harvest and brought instant unemployment on a vast scale and with devastating consequences. For all the vaunted labor requirements of the latifundia system, plantations were incapable of providing employment to all workers all year long. A labor surplus in the rural population resulted, which caused destitution among workers where once there existed self-sufficiency and subsistence among farmers.

Farmers earlier tied to the land, preoccupied year round with matters of planting, harvesting, and marketing, acquired a new mobility once they became wage laborers. Without work, the newly mobile ex-farmer could take—and was often required to take—temporary leave of field and family in pursuit of

promovido para reunir los estados de hechos criminales cometidos por partidas de bandoleros en las seis provincias de la Isla," n.d., ANC, Fondo de Asuntos Políticos, file 82, no. 1.

[52] U.S. War Department, *Informe sobre el censo de Cuba, 1899* (Washington, D.C., 1899), 554–56; and Robert B. Hoernal, "Sugar and Social Change in Oriente, Cuba, 1898–1946," *Journal of Latin American Studies*, 8 (1976): 221–26. Also see Iglesias, "Algunos aspectos de la distribución," 126.

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alternative sources of income. Banditry offered a way out of privation, an adequate if hazardous means of subsistence. "Laborers on the plantations," the U.S. consul in Matanzas reported in the summer of 1884, "have turned into highwaymen."[53]

Brigandage had existed intermittently throughout western Cuba from the beginning of the nineteenth century. Indeed, fugitive outcasts had operated outside the law almost as far back as the century of conquest.[54] But the banditry of the late nineteenth century differed in marked ways from older forms of brigandage. Banditry after Zanjón was a localized phenomenon and did not affect all regions of Cuba equally. In those districts where banditry flourished, it was something of a ubiquitous presence—ambiguous and amorphous but persistent and proliferating. It was, in fact, rebellion—more in response to local injustice than to colonial inequities, more parochial and communal than patriotic and national. It flourished through communal collaboration and local countenance. In form and function, at its source and in its scope, banditry gave expression to popular rejection of the new order. It was an assault against property, property owners, and inevitably the authorities charged with their defense. Bandits came from among those expelled from the land or those seeking escape from destitution. Banditry was different, too, in that bandits operated in comparatively large and close-knit groups and not as lone highwaymen. The size of the fifteen distinct bands operating in post-Zanjón Cuba averaged between fifteen and thirty men, bound together by shared values in common activities against shared sources of oppression—a collective form of peasant dissidence.[55]

DEVELOPMENTS IN WESTERN CUBA after Zanjón conform generally to those conditions that Eric Hobsbawm has suggested engender a specific form of rural protest identified as social banditry: peasants transformed into outlaws of a special type who enjoy the support of local residents. Social bandits operate with the acquiescence, often with the approval, of peasant communities and always within the bounds of the moral order of rural society. They are perceived as heroes, who plunder from the prosperous to provide for the poor. They are avengers, redressing grievances and righting wrongs. Banditry as an expression of premodern protest, Hobsbawm suggested, reaches epidemic proportions during times of pauperization and economic crisis. More specifically, banditry "may reflect the disruption of an entire society, the rise of new classes and social structures, the resistance of entire communities or peoples against the destruction of its way of life."[56] Peasants take up banditry, Hobsbawm wrote elsewhere, to protest oppression and impoverishment, as an outcry against the advent of agrarian capitalism

[53] Vickers to Davis, August 27, 1884, Despatches/Matanzas.

[54] See Francisco López Leiva, *El bandolerismo en Cuba (contribución al estudio de esta plaga social)* (Havana, 1930), 7–22.

[55] Tesifonte Gallego, *La insurrección cubana* (Madrid, 1897), 198–200; Alvaro de la Iglesia y Santos, *Manuel García (el Rey de los Campos de Cuba): Su vida y sus hechos* (Havana, 1895), 18, 34–37; and López Leiva, *El bandolerismo en Cuba*, 24.

[56] Hobsbawm, *Bandits* (New York, 1969), 18.

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that comes to them from the outside, "insidiously by the operation of economic forces which they do not understand and over which they have no control."[57]

Bandits operated across western Cuba throughout the 1880s and 1890s, involving at the height of rural unrest an estimated eight hundred men.[58] A few former soldiers and former slaves joined the displaced farmers and landless laborers in outlaw bands. Bandits ranged in age between twenty-five and thirty-five years old; some were younger but few older.[59] Bands under Juan Vento, José Inocencio Sosa ("Gallo Sosa"), and Manuel García ("el rey de los campos de Cuba"), the most famous leader, operated in Havana province. Victoriano and Luis Machín dominated the Vuelta Bajo region in Pinar del Río. José Plasencia, José "Matagás" Alvarez, Nicanor Duarte, Regino Alfonso, Desiderio and Nicasio Matos, and Aurelio Sanabria ranged across the interior districts of Matanzas province. In Santa Clara, Florentino Rodríguez and Bruno Gutiérrez eluded Spanish authorities for more than a decade.

During the late 1880s and early 1890s no planter or plantation was safe. Attacks against planters expressed first the social content and later the political context of peasant grievances. Planters were subjected with increasing frequency to kidnapping and ransom demands. Between 1883 and 1888, government authorities in Matanzas province reported nineteen abductions involving ransom payments in excess of seventy thousand pesos in gold.[60] Similar conditions were reported in Havana province[61] and Santa Clara.[62] Between November 1886 and May 1887, the kidnapping of planters in the provinces of Pinar del Río, Havana, and Matanzas occurred at the rate of one per month.[63] The patriarchs of some of the most prestigious and powerful planter families, including Nicolas Pérez Artiles, Antonio Galíndez de Aldama, Pedro Sardiña, and Antonio Fernández de Castro, fell captive to kidnappers and were released only after the delivery of substantial ransom payments.[64]

Attacks against property also increased. Bandits attacked plantations, destroyed buildings, sacked supply stores, and confiscated livestock.[65] But most of all, and

[57] Hobsbawm, *Primitive Rebels* (New York, 1965), 3. Also see Anton Blok, "The Peasant and the Brigand: Social Banditry Reconsidered," *Comparative Studies in Society and History*, 14 (1972): 494–503.

[58] See Ballou, *Due South*, 97.

[59] For brief biographical sketches of some of the more prominent bandits, see Eduardo Varela Zequeira and Arturo Mora y Varona, *Los bandidos de Cuba* (2nd edn., Havana, 1891), 25–63; and Iglesia y Santos, *Manuel García*, 34–37.

[60] "Expediente sobre datos de bandolerismo en Matanzas," 1888, ANC, Fondo de Asuntos Políticos, file 82, no. 5; and Antonio Pirala, *España y la regencia: anales de diez y seis años (1885–1902)*, 3 vols. (Madrid, 1904–07), 2: 250–53.

[61] "Expediente promovido para reunir datos de bandolerismo de la provincia de La Habana," January 3, 1888, ANC, Fondo de Asuntos Políticos, file 81, no. 21.

[62] "Expediente promovido para reunir datos sobre el bandolerismo de la provincia de Santa Clara," January 9, 1888, ANC, Fondo de Asuntos Políticos, file 80, no. 21.

[63] "Expediente promovido para reunir los estados de hechos criminales cometidos por partidas de bandoleros en las seis provincias de la Isla," n.d., ANC, Fondo de Asuntos Políticos, file 82, no. 1.

[64] See "Expediente promovido con motivo del secuestro del Exmo. Sor. D. Antonio Galíndez de Aldama," 1888, ANC, Fondo de Asuntos Políticos, file 82, no. 4; and "Telegrama sobre el secuestro del hacendado Antonio Fernández de Castro, por la partida de Manuel García," September 22, 1894, ANC, Fondo de Asuntos Políticos, file 84, no. 7. Also see Iglesia y Santos, *Manuel García*, 107–09.

[65] For a detailed account of one attack against the town of Nueva Paz in Havana province, see *Diario de la Marina*, November 14, 1889.

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most often, they torched the cane fields. Miles of open, unprotected fields made arson all but impossible to prevent. The vulnerability of cane fields to the torch made arson easily the most common, and frequent, expression of peasant discontent. During the harvest of 1891, which lasted about one hundred fifty days, Spanish authorities reported a total of 461 cane field fires.[66]

These conditions made planters vulnerable to still another type of harassment—extortion. Bandits routinely exacted annual tributes from planters in exchange for guarantees of security. Manuel García was particularly successful in collecting levies.[67] Indeed, many planters settled directly with bandits rather than risk property damage or interruption of the harvest. American planter Edwin F. Atkins later recalled that many landowners in the Cienfuegos region paid García, although Atkins preferred "to incur the expense of maintaining a strong field guard to paying tribute."[68]

Beginning in the 1890s, García expanded his operations to include the Ferrocarriles Unidos de La Habana, the major railroad service in Havana province and the principal source of transportation of sugar and plantation equipment. The railroad shared one fateful similarity with the plantations—miles of unprotected property in the form of tracks and isolated stations. When, in 1891, officials of Ferrocarriles Unidos ignored the demand for ten thousand pesos in annual tribute, García attacked the Ferrocarriles Unidos, alternately derailing trains, destroying rail stations, and assaulting railroad crews.[69]

Banditry made the Cuban countryside something of a war zone. Travel in rural Cuba soon became hazardous, and few travelers of means ventured very far into the interior without adequate security. "There are many lawless people—banditti, in fact," commented Ballou, "who make war for plunder both upon native and foreign travelers, even resorting in some cases to holding prisoners for ransoms. . . . It is, therefore, necessary to carry arms for self-defense upon the roads in some parts of the island, and even the countrymen wear swords when bringing produce to market. Residents having occasion to go any distance inland take a well-armed guard with them, to prevent being molested by the desperate refugees who lurk in the hill country."[70] Vickers reported from Matanzas in 1884 "an alarming increase of brigandage all over the Island. In a year it has been unsafe to go into the country unless armed like a pirate, and even then one is constantly

[66] Camilo G. Polavieja, *Relación documentada de mi política en Cuba* (Madrid, 1898), 212. Governor General Polavieja claimed that most of these fires were accidental, and undoubtedly many were. During the 1892 harvest, however, an American correspondent in Cuba reported, "Without doubt . . . outlaws have been the cause of the frequent fires among the cane fields which are daily reported"; *New York Times*, April 4, 1892.

[67] See Iglesia y Santos, *Manuel García*, 55.

[68] Atkins, *Sixty Years in Cuba* (Cambridge, Mass., 1926), 150–51.

[69] *Diario de la Marina*, August 9, 1890; "Expediente promovido para reunir los datos relativos a hechos bandalicos entre los cuales figuran el incendio del paradero del ferrocarril de Quivicán, por la partida de bandidos capitaneados por Manuel García," 1890, ANC, Fondo de Asuntos Políticos, file 82, no. 24; and "Expediente promovido por consecuencia de oficio de la Empresa Ferrocarriles Unidos de La Habana, dando cuenta de que en el kilómetro 64, de los bandidos de la partida de Manuel García, levantaron un carril, descarrilando un tren de carga e hicieron fuegos al maquinista y conductor," 1890, ANC, Fondo de Asuntos Políticos, file 82, no. 24.

[70] Ballou, *Due South*, 63–64.

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in danger of assassination. Owners of estates cannot visit their plantations without running the risk of kidnapping, and the cities are becoming as dangerous as the country."[71]

More than the safety of travelers was at risk. Deepening social tensions in western Cuba threatened the most productive agricultural regions of the island with ruin. Trade and commerce suffered; transportation was disrupted. And there was no evidence to suggest that order would soon return. On the contrary, incidents of banditry increased in frequency and grew in audacity. By the 1890s, sugar estates once immune from banditry by their nearness to the capital were increasingly under attack. In January 1893, Ignacio Herrera, a prominent planter and the son of the count of Barreto, was kidnapped within the Havana city limits and held for ten thousand pesos in ransom. In 1895, the sugar mill Portugalete, located almost within view of Havana, was attacked and destroyed by Manuel García.[72] The U.S. consul in Havana described the growing sense of urgency among local residents. "Small bands of marauders have often existed in Cuba," wrote Ramon O. Williams as early as 1887, "but heretofore the Government has always succeeded in speedily capturing or dispersing them. Never before, however, have they been so bold and defiant as now."[73] The Havana daily *Diario de la Marina* similarly denounced the "lamentable plague" of banditry. The increasing incidents of planter kidnappings in particular aroused editorial ire and prompted the newspaper to demand quick official action. "The evil is a very grave one, and it is necessary to stop it, for the personal tranquillity of many families and of thousands of persons in the country is involved, all of whom ask for support and protection against these thieves and murderers. . . . Special energy is required for the extinction of this plague."[74]

But it was not for want of government efforts that banditry flourished. Indeed, it may very well have increased as a result of those efforts. Few other issues between 1878 and 1895 so preoccupied colonial authorities, for social unrest in the countryside had far-reaching political and economic implications. The assault against the planter bourgeoisie struck at the linchpin of the colonial system, threatening the vital interests of the class upon whose continued collaboration Spain depended for successful rule in Cuba. The inability to defend the persons and property of the planter class frightened administrators, already on the defensive against growing political dissent and deepening economic dislocation. The mere existence of bandit groups, particularly the number that flourished in post-Zanjón Cuba, weakened the moral authority of colonial government and undermined the prestige of Spanish administration. The authorities, beleaguered planters complained, could not provide even minimal guarantees of security to the producing classes, which made the prospect of economic revival uncertain.[75]

[71] Vickers to Davis, August 27, 1884, Despatches/Matanzas.
[72] Gallego, *La insurrección cubana*, 247.
[73] Ramon O. Williams to James D. Porter, March 26, 1887, Despatches/Havana.
[74] *Diario de la Marina*, March 24, 1887.
[75] Governor General Polavieja was concerned with ending banditry not only to restore stability to rural Cuba but also to restore credibility to colonial government. Throughout his administration he feared that the persistence of outlawry would undermine Spanish authority on the island. See Polavieja, *Relación documentada de mi política en Cuba*, 85, 195–97.

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Disorder in the rich agricultural zones of western Cuba, moreover, threatened to thwart efforts to recover Cuba's former primacy in sugar production and thereby threatened the planter class with ruin.[76]

These conditions encouraged annexationist sentiment among planters, who increasingly believed that union with the United States would bring the security and order absent under Spanish rule. Planters in increasing numbers were experiencing a crisis of confidence, and more than a few publicly questioned the efficacy of remaining a Spanish colony. These were portentous developments, and no one misconstrued the portents. "Many are already talking of resistance and annexation to the United States," reported Vickers from Matanzas as early as 1884, "not that they love Spain less, but their interests more."[77] "Annexation to the United States," the Havana daily *El Popular* predicted confidently in 1887, "would make Cuba a rich, enlightened, and tranquil country."[78] Wrote the U.S. consul in Havana, "A great many persons here, who years ago hooted the idea of annexation to the United States now advocate it, as the only remedy for the preservation of public order and the attainment of future commercial prosperity in the island."[79]

The government was not slow to respond to rural lawlessness. Across the western interior, the government military presence increased. As early as 1884, in an attempt to provide better protection to planters in Matanzas during the harvest, colonial authorities augmented the number of Guardia Civil detachments.[80] In 1889, Spain established a Rural Guard Corps, organizing the unit into a network of permanent rural outposts throughout the sugar zones.[81] In the same year, Governor General Manuel Salamanca ordered all Guardia Civil posts on the island linked together by telephone to facilitate communication and coordinate operations against outlaws. Salamanca subsequently authorized the establishment of telegraph lines between the larger sugar estates and provincial headquarters of the Guardia Civil.[82] Government efforts also included more surveillance and collection and analysis of intelligence. The authorities continued to alternate offers of amnesty with the posting of generous rewards for information leading to the capture of known fugitives.[83]

The most ambitious government undertaking took the form of full-scale military operations. In 1888 the most formidable Spanish military offensive since the end of the Ten Years War was launched in response to a sudden increase in kidnappings of planters and, consequently, rising public impatience. Governor

[76] López Leiva, *El bandolerismo en Cuba*, 28–29.

[77] Vickers to Davis, July 2, 1884, Despatches/Matanzas.

[78] *El Popular*, February 11, 1887.

[79] Ramon O. Williams to James D. Porter, May 18, 1887, Despatches/Havana. For a discussion of annexationist sentiment during these years, see José Ignacio Rodríguez, *Estudio histórico sobre el origen, desenvolvimiento y manifestaciones prácticas de la idea de la anexión de la Isla de Cuba a los Estados Unidos de América* (Havana, 1900), 243–86.

[80] *Diario de la Marina*, December 24, 1884.

[81] "Expediente promovido para crear en esta Isla, el Cuerpo de Guardias Rurales," 1889–90, ANC, Fondo de Asuntos Políticos, file 82, no. 19.

[82] Gobierno de la Isla de Cuba, "Decreto," August 8, 1889, ANC, Fondo Gobierno General, file 584, no. 28863.

[83] *La Lucha*, April 17, 1889; and Francisco Figueras, *Cuba y su evolución colonial* (Havana, 1907), 289–90.

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Sabas Marín proclaimed a state of war in the four western provinces of Pinar del Río, Havana, Matanzas, and Santa Clara, placing the administration of justice entirely under the jurisdiction of military authorities.[84] The offensive continued through 1889 without much effect and was subsequently ended during the brief administration of Governor Manuel Salamanca. But, with the appointment of Camilo G. Polavieja as governor general in 1890, military operations resumed. Within weeks of his arrival in Havana, Polavieja mounted a second and even more formidable offensive against bandits. Some ten thousand troops were amassed in major but again ultimately futile military operations.[85]

Certainly not all government efforts were ineffective. Many bandits were captured by the police and killed in military operations.[86] Indeed, between 1889 and 1891, some of the most prominent bandit leaders, including Sixto Valera, Andrés Santana, and Domingo Montolongo, were captured by the authorities. Public executions were revived, ending a longstanding moratorium on capital punishment—evidence of Spanish determination to end banditry. They were calculated to give maximum publicity to the meager progress made by the government—a public demonstration to calm public disquiet. Between 1889 and 1891, hangings proceeded at the rate of one a month.[87]

But success against individual bandits in the field and convictions in the courts failed to establish order in the provinces. Outlaw bands were neither defeated nor dispersed. After almost ten years, and nearly as many different colonial administrations, the mightiest of Spanish efforts had failed to end what was officially referred to as "the plague." Banditry was a condition that would not go away. As colonial officials knew only too well, military authorities faced more than the task of catching outlaws. They were engaged in a low-level war, not only against bandits but also against communities of thousands of peasants and laborers who served as the natural allies and loyal accomplices of fugitive bands. Either by passive acquiescence or active participation, local residents formed a complex support network, involving men, women, and children who provided outlaws with aid and alibis, protection and provisions, sanctuary and support. In the dispersed villages

[84] Sabas Marín, "Bando," July 3, 1888, ANC, Fondo Gobierno General, file 584, no. 28863.

[85] "Expediente promovido para reunir las disposiciones que se dicten sobre bandolerismo durante el mando del Excmo. Sor. Gobernador General D. Camilo Polavieja," 1890, ANC, Fondo de Asuntos Políticos, file 82, no. 26. Also see Polavieja, *Relación documentada de mi política en Cuba*, 199–200; and López Leiva, *El bandolerismo en Cuba*, 22–25.

[86] See "Relación nominal de bandidos muertos por la fuerza pública desde 1ro julio de 1887 a 28 marzo de 1888," March 28, 1888, ANC, Fondo de Asuntos Políticos, file 81, no. 18.

[87] See Iglesia y Santos, *Manuel García*, 170; and Polavieja, *Relación documentada de mi política en Cuba*, 211–12. The publicity value of public executions was underscored by the following press despatch: "Deep alarm reigns all over the country in consequence of the daily increase of all sorts of crimes, the perpetrators being encouraged in their actions by the complete immunity they enjoy. In spite of daily assassinations, the public never learns whether the assassins are punished, and an execution has not taken place since the days when political prisoners were occasionally garroted." *New York Times*, July 12, 1883. The bandits captured and subsequently executed by Spanish authorities between 1889 and 1891 included Victoriano Machín; Cristobal Fernández Delgado; Eusebio Moreno y Suárez; Joaquín Alemán; Federico Acosta; José Manuel Martín Pérez; Dionisio Guzmán Pérez; Manuel and José de León Ortiz; Nicanor Duarte Ramos; Venancio, José, and Carmelo Díaz Ramos; Felipe González López; Francisco Paz; Pedro Boitel; Pedro Macías Ortall; José Estrauman Daria; Valentín González López; Guillermo Pérez Cruz; José Sánchez Ortega; Pablo Cantero; and Teodor Galano.

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and hamlets of rural Cuba the military's task became impossible. Bandits were themselves local men, whose familiarity with the remote terrain in which they operated accounted in part for the ease with which they eluded the authorities.

More important, however, were the ties of kinship and friendship that bound outlaws to the rural communities. Bandit groups represented a cross section of the people among whom they lived. They shared common experiences and traditions but most of all shared similar grievances. They gave form to local resistance to the new socioeconomic order and the old political-military system. Attacks were directed principally against sources of local oppression, and each deed had symbolic content. "The peasants continue protecting Manuel García," an exasperated Polavieja reported in 1891, "for they see in him, not the bandit, but the partisan up in arms against Spain."[88]

Bandits were supported generally by entire communities but especially by those peasants who were themselves caught mid-way between the peasantry and the proletariat—forced inexorably toward wage labor but still clinging to the land.[89] The abduction of planters, the attacks against the plantations, and the assaults against the railroads were actions readily understood by rural communities. On occasion, bandits passed the spoils of war on to the rural population. Manuel García frequently used ransom money to buy food, supplies, and equipment for peasants and rural workers.[90] This gesture may have been motivated as much by practical considerations as political conviction, for it served to foster goodwill among the people upon whose support bandits depended for survival.[91] And the villagers responded in kind. About García, runaway slave Esteban Montejo later recalled: "He was a friend of the *guajiro* [peasant], a real friend. If they ever saw the police getting close to a place where Manuel was, they used to take their trousers off and hang them on a rope waist down. This was the signal for Manuel to get moving, and that must be why he lived so long as an outlaw."[92]

Colonial authorities were not unaware of the magnitude of this collaboration. Nor were they unmindful of its implications. Indeed, Spanish intelligence reports provide a comprehensive and detailed account of the extent of the aid that farmers gave to fugitives. One military intelligence report from the town of Madruga in eastern Havana province contains a lengthy list of local residents who assisted bandit leaders. These included Rafael Almeida, who sheltered the bandit

[88] Polavieja, *Relación documentada de mi política en Cuba*, 205.

[89] Guillermo R. Fort, chief of operations, Guardia Civil, Madruga, "Relación," December 19, 1894, ANC, Fondo Gobierno General, file 584, no. 28863.

[90] Esteban Montejo, *Autobiography of a Runaway Slave*, trans. Jocasta Innes and ed. Miguel Barnet (London, 1968), 113. Also see Eduardo Zamacois, *La alegría de andar* (Madrid, 1920), 153–54; and Manuel García to José Lachambre, n.d., in Varela Zequerira and Mora y Varona, *Los bandidos de Cuba*, 212.

[91] This practice was one way that Brazilian bandits garnered support from the peasantry. See Linda Lewin, "The Oligarchical Limitations of Social Banditry in Brazil: The Case of the 'Good' Thief Antonio Silvino," *Past and Present*, 82 (1979): 137–38.

[92] Montejo, *Autobiography of a Runaway Slave*, 119. Montejo's account is corroborated by the testimony of Lorenzo Yanes, a farmer in Melena del Sur, who at the age of ninety-two recounted several instances of peasant support for Manuel García. See Lorenzo Yanes and René Batista Moreno, "Retratos de una vida hazañera," *Bohemia*, January 22, 1971, pp. 98–99. Also see López Leiva, *El bandolerismo en Cuba*, 27–29.

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Asunción Muerte; Victoriano Jacomino, who provided bandit José Plasencia changes of clothing; Miguel Mesa, who supplied Manuel García with food and horses; Joaquín Chávez, who regularly gave bandits shelter, supplies, and fresh horses; Manuel Hernández, whose home was a meeting place for several bandit groups; José Mario Brito, who acted as a purchasing agent for supplies; Severino Méndez and Matías Domínguez Fundora, who served as intermediaries during kidnapping negotiations and ransom transactions; Juan Luis Bello, who acted as messenger among various bandit groups; Tomas Hernández, who sheltered Manuel García, José Plasencia, Vicente García, and Gallo Sosa and served as a collector of ransom funds for Antonio Santana. All had a role in the rural drama. Some provided intelligence information concerning the strength of local army units, and others reported the movement of government forces.[93] But all steadfastly professed ignorance of the activities or whereabouts of the outlaws. "He is protected absolutely by the country people," Polavieja wrote of Manuel García.[94]

Nor was the collaboration of the country people restricted to passive support. Occasionally some served as auxiliary members of an outlaw band, participating in special operations that required augmented forces. "Every group of bandits," Polavieja later wrote, "counted upon a certain number of peasants who joined them armed for important actions, and who subsequently returned to their homes as peaceful citizens and ignorant of everything."[95] This practice was especially prevalent during the dead season, when the unemployed sought relief from the combined effects of idleness and indigence.

The government's fight, hence, was not primarily with the outlaw bands but with the communities that aided them. Increasingly, and inevitably, the full weight of government frustration fell on the country people suspected of collaborating with fugitives. The imposition of martial law in 1888 and 1890 and subsequent military operations were directed as much against the local rural population as against the bandits themselves, who, in any case, routinely eluded even the most widespread military operations. During periods of martial law peasants and rural workers were arrested and tried by courts martial. Arrests were indiscriminate. Peasants detained for questioning disappeared. Some were beaten to death; others were subjected to the *ley de fuga*: "shot while trying to escape." Survivors told of beatings and torture. The Guardia Civil became a source of terror, deepening the estrangement between the country people and colonial authorities.[96] Each military campaign meant the disruption of local life, and communities dispersed in anticipation of new and sustained periods of government operations. Village

[93] Fort, "Relación," December 19, 1894, ANC, Fondo Gobierno General, file 584, no. 28863. Another intelligence report of comparable detail is available for the town of Quivicán in central Havana province. See Justo Pardo González, Operations, Guardia Civil, Quivicán, "Relación," December 18, 1894, ANC, Fondo Gobierno General, file 584, no. 28863; and Varela Zequeira and Mora y Varona, *Los bandidos de Cuba*, 44.

[94] Polavieja, *Relación documentada de mi política en Cuba*, 211.

[95] *Ibid.*, 88. Throughout his memoir, Polavieja complained of peasant support of bandits and how this collaboration made the Spanish task all but impossible. See *Ibid.*, 87, 195, 213.

[96] Luis Estévez y Romero, *Desde el Zanjón hasta Baire*, 1 (2d edn., Havana, 1975), 311; and Portuondo del Prado, *Historia de Cuba*, 502.

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marketplaces were destroyed, and local trade and agriculture suffered. The homes of suspected peasant collaborators were burned and razed. "The government has had at times as many as six thousand troops," one Cuban wrote, "besides the rural police [Orden Público] and volunteers, in pursuit of these brigands, and yet for every bandit captured or killed, the newspapers are filled with complaints from poor farmers who are beaten and insulted as harborers of criminals. All this show of force has been another source of squandering and tension as a result of the summary executions in unpopulated regions, on the pretext that suspects resisted arrest or tried to escape."[97] Villagers abandoned their homes and fled deeper into the remote interior in search of sanctuary. Some sought temporary safety with outlaw bands. Indeed, in 1889, Manuel García announced that government military operations had increased his band of nineteen to a battalion of four hundred.[98]

The failure of the colonial government to defeat bandits led to greater efforts to dominate the peasants who supported them. Responsibility and punishment for the actions of bandits were transferred to the country people, without whom, authorities insisted, fugitives could not long sustain their depradations. And, increasingly, it was against the rural communities that authorities directed their actions. Many country people, the civil governor of Havana province, Luis Alonso Martín, warned in November 1887, "either through culpable complicity, or from lack of energy and courage, contribute to making more difficult, when not completely defeating, the action of the authorities, by maliciously concealing the passage of bandits through the fields, or reporting it too late, and in many cases withholding information of important matters." Martín proclaimed that all persons had an "imperative duty" to report the presence or passage of fugitives in local communities. Delay in communicating information, moreover, "if it exceeds the time deemed necessary in the judgment of the authorities," would be construed as "shielding the malefactors," and appropriate action would be taken.[99] In April 1888, only days after the kidnapping of planter Antonio Galíndez de Aldama, the Havana daily *Voz de Cuba* exhorted the government to prosecute all participants in the crime, particularly those who sheltered the outlaws. "Let the penalty for kidnapping," the editorial demanded, "be executed not only upon the bandits, but upon their harborers and accomplices, and upon all engaged in this detestable plot to ruin our agricultural wealth."[100] In proclaiming martial law in 1888, Governor Sabas Marín pledged to proceed with an equally "strong hand against the leaders, accomplices, and harborers of these crimes." Enjoining his field commanders to move against communities suspected of providing aid and comfort to bandits, Sabas Marín vowed: "I am resolved to exact of them the responsibility they incur, if they neglect to lend their moral and material support to the government, which their duty as citizens imposes upon them." Indeed, it was

[97] Merchán, *Cuba, justificación de sus guerras de independencia*, 88.
[98] Varela Zequeira and Mora y Varona, *Los bandidos de Cuba*, 79.
[99] Luis Alonso Martín, "Circular from the Civil Governor of Havana Concerning the Suppression of Highway Robbery," November 14, 1887, in Cabrera, *Cuba and the Cubans*, 305–06.
[100] *Voz de Cuba*, April 8, 1888.

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precisely the growing collaboration between bandits and villagers, Sabas Marín explained, that required the government to impose martial law. "The aid given . . . in a part of the island by a few deluded persons," he stated, "converting themselves into aiders and abettors, compels the adoption of extraordinary measures to put an end to disorders."[101]

Governor Manuel Salamanca demanded more legal action in the courts and enjoined government attorneys (*fiscales*) to prosecute vigorously all suspects charged with aiding bandits. "Without doubt," Salamanca predicted in 1889, "the active and well-directed efforts of the military forces against the bandits that infest the rural districts will have little effect if the attorneys entrusted with their prosecution do not display all the zeal and intelligence necessary not only for the discovery of crimes but also for the bringing of accomplices, aiders, and abettors to trial, regardless of their social rank or motive of relationships with those subject to the action of the court and with those whom they help escape punishment and thereby directly or indirectly impede the administration of justice." Salamanca concluded:

> It is of great importance that these functionaries should take into consideration that one of the most efficient means for the destruction of banditry and the prevention of bandits from having their own way is the punishment of all persons who lend them help, and the exaction of due responsibility of those who sympathize with them, for it is an undeniable fact that if the bandit who respects neither life nor property finds no assistance in the districts where he exercises his criminal profession, nor finds even shelter afforded by the sentiment of indifference toward his acts, he would feel the dangers of his isolation and in a short time would be obliged to abandon the fields of his habitual incursion.[102]

Not all peasants and workers collaborated with bandits willingly. Any peasant discovered by the authorities to have fed and sheltered bandits would have been wise to claim coercion, but some aided the outlaws only under duress. "Parties of these bandits," wrote U.S. Vice Consul Henry A. Ehringer in Cienfuegos, "numbering from 5 to 10 go about the country relying on the sympathy or toleration of the inhabitants of whom they exact food and horses, extorted in many cases by threats of personal violence or destruction of their property." Ehringer continued:

> A party visits the house of a countryman and demands food, money, or horses. The person called upon dares not refuse them. Afterwards there arrives a party of troops or police in pursuit of these outlaws, and hearing that they have been there, they arrest the already suffering countryman on the charge of having given aid and shelter to the bandits. In many cases taking into their own hands the punishment of those persons who acted from self-preservation.[103]

The bandits' use of coercion was often disregarded by the authorities. As a result, peasants faced a dilemma. Those known to have assisted the government were

[101] Sabas Marín, "Circular," *Diario de la Marina*, April 19, 1888. Also see *La Lucha*, April 17, 1888; and Sabas Marín, "Bando," July 3, 1888, ANC, Fondo Gobierno General, file 584, no. 28863.
[102] *Gaceta de La Habana*, April 3, 1889.
[103] Ehringer to James D. Porter, December 29, 1886, Despatches/Cienfuegos.

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frequently the victims of assassination—a lesson not lost on country people.[104] The authorities that could not protect the powerful planters could hardly be expected to protect the powerless peasant. In a curious way, the crisis of confidence that gripped the propertied also affected the propertyless. The colonial regime revealed itself incapable of providing even minimal guarantees for persons and property. It had become irrelevant to the reality of Cuban society.

BANDITRY PERSISTED through early 1895, when in February Cubans across the island rose up in the name of Cuba Libre. The forces released during the Ten Years War and the economic dislocation and social distress in post-Zanjón Cuba had contributed to deepening colonial discontent and served as the source for the renewal of armed struggle. The new separatist movement was, however, dissimilar in character and different in composition from the earlier rebellion. One difference was planning. Another was preparation. Furthermore, in 1895 the Cubans were united and organized. A broad coalition of Cubans had organized under the Cuban Revolution party (PRC), sustained during times of enormous adversity by an indissoluble commitment that by the late nineteenth century had assumed fully the proportions of a political faith.

But neither preparation nor organization, however vital to the success of Cuban separatists, was the decisive difference in 1895. Rather, the ideological content of separatist thought and the social origins of separatist leadership were the key factors. Cuba had changed from the "Grito de Yara" in 1868 to the "Grito de Baire" in 1895, and in the intervening years the sources of Cuban grievances no longer emanated exclusively from the rule of the distant European metropolis. By the late nineteenth century, Spain was neither the principal beneficiary nor the primary benefactor of the colony. Inequity in 1895 had a peculiarly home-grown quality. That the sources of oppression in Cuba were more internal than external, more social than political, served as the central premise around which separatism took shape during the 1880s and 1890s. The separatists were committed to more than independence. The movement subsumed a social imperative into a vision of a free Cuba. What was different in 1895 was the recognition that inequity was principally caused not by Spanish political rule, for which independence was the obvious solution, but by the Cuban social system, for which the transformation of Cuban society was the only remedy. Cubans continued to speak of independence, but they now spoke too of war as a method of redemption and a means of social revolution. Political separatism had expanded into revolutionary populism, committed as much to ending colonial relationships within the colony as to ending the colonial connection with the metropolis. The separatist enterprise was conceived as both

[104] Polavieja wrote of one incident in which a peasant couple dutifully reported a visit from Manuel García to the authorities. The couple was subsequently killed by the bandit. The best that Polavieja could do was to secure public charity for the orphaned children. On the body of the dead man, García had left the following note: "Hernández had been my friend since boyhood. I killed him because he tried to deliver me to the Guards. I have never before killed a woman, but I killed his wife because she induced him to betray me. I hurt only those who hurt me." Polavieja, *Relación documentada de mi política en Cuba*, 206–07; and *New York Times*, October 6, 1891.

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a rebellion against Spanish political rule and revolution against the Cuban social order.

The vision of Cuba Libre remained admittedly imprecise. Cubans spoke more to aspiration than action, promise rather than program. Concerns were expressed in thematic rather than programmatic terms. The separatist leadership did not develop a program as much as it identified the problems and committed the future republic to their resolution. But these vague commitments established the articles of faith around which Cubans gathered. Separatism evolved into an endeavor to change Cuban politics fundamentally by creating new ways of mobilizing and sharing political power. The sources of Cuban discontent—social, economic, political, racial, historic—converged into a radical movement of enormous force. To the historic objective of national liberation was added a social imperative of national revolution, and a movement dedicated to the establishment of a new nation became a movement devoted to shaping a new society.

Different, too, were the social origins of separatist leadership. A new constituency organized around Cuba Libre: the politically powerless, the socially displaced, and the destitute—Cubans for whom armed struggled offered the means through which to redress historic grievances against the colonial regime and its local defenders. A dispossessed peasantry, an expatriate proletariat, former slaves and farmers responded to the summons to arms. Many were men of color. Some 40 percent of the senior commissioned ranks of the Liberation Army was made up of Afro-Cubans.[105] These were Cubans for whom the old regime was as much a social anathema as it was a political anachronism. They had committed themselves to a movement that promised not only to free them from the old oppression but also to give them a new place in society, a new government they would control, and a new nation to belong to. The Cuban emigre communities in Key West, Tampa, Ocala, and Jacksonville also gave focus and force to the stirrings of Cuba Libre. The expatriate proletariat was a vital constituency around which the PRC organized.[106]

As banditry had served to express local grievances, thereby keeping the rural population in more or less a state of continuous resistance to the colonial regime, it had also transformed rural Cuba into a theater of permanent military operations. Local discontent with government military actions and the post-Zanjón economic order—which generalized into resistance against the colonial political system—enhanced enormously the appeal of separatism. Local communities soon found their grievances with the old political regime overtaking their discontent with the new agrarian regimen. Independence offered one solution to both problems.

In the early 1890s, bandit activities had acquired distinctive separatist undercurrents. Individual bandit chieftains found the passage from social protest to

[105] See Matías Duque, *Nuestra patria* (Havana, 1923), 143; Kenneth F. Kiple, *Blacks in Colonial Cuba, 1774–1889* (Gainesville, Fla., 1976), 81; Rafael Fermoselle, *Política y color en Cuba* (Montevideo, 1974), 26; and Donna M. Wolf, "The Cuban 'Gente de Color' and the Independence Movement, 1879–1895," *Revista/Review Interamericana*, 5 (1975–76): 403–21.

[106] Louis A. Pérez, Jr., "Cubans in Tampa: From Exiles to Immigrants, 1892–1901," *Florida Historical Quarterly*, 53 (1974): 129–40.

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political rebellion easy, if not inevitable. Manuel García was an ardent supporter of Cuba Libre. He had been one of those criminals who earlier had accepted a Spanish subsidy to emigrate to Florida. And it was in the course of his two years as a cigar worker in Key West that García's conversion to the separatist faith occurred. When he returned to Cuba in 1888, he did so as an agent of the Revolutionary Club of Key West and often invoked the slogans of Cuba Libre during his assaults against property.[107]

During the 1890s, too, funds obtained by bandits from ransom payments increasingly found their way into separatist coffers. Much of the money collected by García during these years served to support the revolutionary activities of his former associates in Key West. The ransom obtained for the release of planter Antonio Fernández de Castro in 1894 was donated to PRC organizers in Havana. During the 1890s an estimated seventy-five thousand pesos were given to revolutionary organizations in the United States. Money was used, too, to buy arms, ammunition, and supplies, all of which placed entire districts of rural Cuba on a war footing. Between them, Manuel García and José "Matagás" Alvarez managed to collect some five hundred arms.[108]

Banditry gave way to a war of liberation, and both offered a means to right old wrongs. Many identified the struggle for independence with the fight for land. Nor was this an incorrect perception. In 1896, in what was tantamount to a sweeping land reform decree, the insurgent army command proclaimed:

> All lands acquired by the Cuban Republic either by conquest or confiscation, except what is employed for governmental purposes, shall be divided among the defenders of the Cuban Republic against Spain, and each shall receive a portion corresponding to the services rendered. . . . All lands, money, or property in any and all forms previously belonging to Spain, to its allies, abettors, or sympathizers, or to any person or corporation acting in the interest of Spain or in any manner disloyal to the Cuban Republic are hereby confiscated, for the benefit of the Cuban Army and of all the defenders of the Cuban Republic.[109]

Furthermore, a change of government, particularly a change to which individual bandit leaders contributed, promised to end the fugitive existence for many. Only a month before the outbreak of the 1895 war, Manuel García acknowledged that his support of the separatist cause was in part motivated by the hope of obtaining from the new republic a pardon allowing him to return to peaceful pursuits.[110]

García was not alone. Scores of outlaw bands passed directly into armed insurgent ranks. José Inocencio Sosa in Havana, José "Matagás" Alvarez in Matanzas, Nicasio Matos in Santa Clara, and Lino Mirabal in Camagüey immediately seconded the call for independence and integrated their followers into the

[107] Varela Zequeira and Mora y Varona, *Los bandidos de Cuba*, 86.

[108] See Octavio Ramón Costa y Blanco, *Juan Gualberto Gómez: Una vida sin sombra* (Havana, 1950), 118–19; Emilio Reverter Delmas, *Cuba española: Reseña histórica de la insurrección cubana en 1895*, 1 (Barcelona, 1897), 3; and Rafael Gutiérrez Fernández, *Los heroes del 24 de febrero* (Havana, 1932), 144–56.

[109] Headquarters of the Army of Liberation, "Proclamation," July 4, 1896, in Joaquín Llaverías y Martínez, ed., *Correspondencia diplomática de la delegación cubana en Nueva York durante la guerra de 1895 a 1898*, 5 (Havana, 1946), 176–77.

[110] Iglesia y Santos, *Manuel García*, 200.

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columns of the Liberation Army.[111] In 1895 the bandits became patriots. By themselves bandits had failed to develop an adequate organizational structure for a struggle of liberation. They possessed neither the capacity for political mobilization nor a program for revolutionary change. But they were successful in sustaining protest. Perhaps more important, bandits represented defense of the traditional agrarian order and hence served to embolden regional resistance and encourage individual rebellion. Their example was emulated by many peasants and workers. These were men who responded to injustice and persecution with armed protest and reprisal, men who rejected the passive social role of the submissive peasant and engaged on its own terms the coercive authority of the state. Their mere existence offered a remedy to oppression. Bandits demonstrated that justice was attainable and that the propertyless were not powerless. When later in 1895 the central columns of the Liberation Army invaded Santa Clara, Matanzas, and Havana, they joined a local uprising already a decade in progress.[112] In ten months Cubans secured what they had failed to accomplish in the Ten Years War: participation of the west in the struggle for independence. A regional stirring for social justice had been transformed into a national struggle for self-determination.

[111] See Oswaldo Morales Patiño, *El Capitán Chino: Teniente coronel Quirino Zamora; Historia de un mambí en la provincia de La Habana* (Havana, 1953), 131; and Miguel Angel Varona Guerrero, *La guerra de independencia de Cuba, 1895–1898*, 2 (Havana, 1946), 836.

[112] For accounts of the invasion, see José Silviera Llorens y Maceo, *Con Maceo en la invasión* (Havana, 1928); Juan J. E. Casasús, *La invasión: Sus antecedentes, sus factores, su finalidad; Estudio crítico-militar* (Havana, 1950); and Francisco Ponte Domínguez, "La invasión," in Emilio Roig de Leuchsenring, ed., *Curso de introducción a la historia de Cuba* (Havana, 1938), 343–50.

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The Shaping of Women's Work in Manufacturing: Guilds, Households, and the State in Central Europe, 1648–1870

JEAN H. QUATAERT

IN 1822 TAILORS IN NAUMBURG AN DER SAALE were embroiled in a serious controversy with Prussian ministers over work. The conflict offers an excellent illustration of the clash of forces that affected the nature of women's work in manufacturing in the early nineteenth century. Ceded to Prussia at the Congress of Vienna in 1815, this Saxon city was now subject to reforms imposed on Prussian society by the government during the Napoleonic era, including the imposition of "free trade" (*Gewerbefreiheit*). According to angry guildsmen, more than seventy untrained seamstresses were engaged in dressmaking in their homes, even though time-honored guild statutes, reaffirmed by the king of Saxony himself in 1780, clearly established guild monopoly over this type of work.[1] These "bunglers" (*Pfuscherinnen*), the guild label for such women, threatened family livelihood, undermined training by apprenticeship, and, most seriously of all, overturned tradition. The list of guild grievances was long and specific, and it was supported by others in cities such as Zeitz and Weisenfels. But the complaints fell on deaf ears. Historians have shown that the Prussian Ministry of State applied the new liberal trade policies in the post-Reform era inconsistently, but this was not true in the conflict between guild and nonguild garment workers.[2] At both the state and provincial levels, government officials continuously sided with the women, and the reasons they gave provoked the ire of guildsmen as much as the competition of the seamstresses did.

The ideas for this article derived from my research on the work and family relationships of seamstresses and weavers during the nineteenth century in Berlin and the Oberlausitz province of Saxony. The research was supported by a fellowship from the National Endowment for the Humanities (1980–81) and summer grants (1982) from the American Council of Learned Societies and the German Academic Exchange Service. I am grateful for the support of these foundations as well as for released time the University of Houston–Clear Lake awarded me in 1984. I wish to thank Douglas Holmes, Mary Jo Maynes, and Gretchen Mieszkowski for useful suggestions that helped clarify themes, organization, and style. I also benefited from the responses of the anonymous readers for the *American Historical Review*, and I thank them for their careful reading of the article.

[1] German Democratic Republic, Zentrales Staatsarchiv II, Merseburg (hereafter, ZStAM), Zivil-Kabinet, 2.2.1, Nr. 27876, Bl. 15–25, April 30, 1822. These documents include a copy of the guild confirmation first granted by the elector of Saxony in 1685.

[2] Wolfram Fischer, *Handwerksrecht und Handwerkswirtschaft um 1800: Studien zur Sozial- und Wirtschaftsverfassung vor der industriellen Revolution* (Berlin, 1955), 62–63; and Ilja Mieck, *Preussische Gewerbepolitik in Berlin 1806–1844* (Berlin, 1965), 54–60.

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In building its case, the government relied on the older notion that the right to work was conditional—a concession granted, modified, or refused by the sovereign "according to times and customs" (*Zeit und Sitte*). But the "times and customs" were now redefined and made compatible with such radical principles as "healthy competition" and benefits for the consuming public. These new precepts justified extending women's right to make clothing for sale—limited earlier to dynamic centers like Berlin in which mercantile policies had loosened guild controls—to all Prussian territories. Government ministers went so far as to declare that right an integral part of the "natural order." Propriety alone and the rule of decency must surely reserve to women the measuring and fitting of women's clothing. Besides, women were expected to sew for their families. But the government's logic eluded the Naumburg guildsmen; their "natural order" had never included such an understanding. Men had traditionally been apprentices in the sewing of women's clothing; tailors had always worked for ladies of the upper classes. It was the employment of women in manufacturing that contravened nature; their proper role was that of housewife or servant in other people's households.[3]

This conflict between guildsmen and Prussian officials was more than a disagreement over the definition of women's proper work. At its heart was an ambiguity over the nature of production. The Romantic age tended to segregate the world of work from that of home, but garment making defied this normative division. Guildsmen could not control work performed in the household. And, as the example of garment making reveals, a large number of households functioned as important centers of production, imparting skills that were transferable to the marketplace. In practical terms, the lines remained blurred between household and market production—between work for personal use, on the one hand, and work for wages, on the other. This ambiguity became a general characteristic of women's work.[4] Conceptually, there was no difficulty in differentiating housework

[3] ZStAM, Zivil-Kabinet, 2.2.1, Nr. 27876, Bl. 11, 15–22. In preindustrial Saxon cities like Naumburg and Zeitz, guild regulations assigned the making of men's and women's clothing exclusively to members; craftsmen chose an apprenticeship in one or the other. This was not the case in Berlin since mercantile policies had already loosened guild controls. There craftsmen monopolized the production of men's clothing, but the sewing of dresses and linens for individual customers was left to free—essentially female—labor. The seamstresses were not permitted to sell their wares on the open market, and only the French couturières could train female apprentices. See W. Zincke, *Sechshundert Jahre Geschichte der Berliner Schneider-Gilde und ihrer Zeit 1288–1888: Eine Festschrift im Auftrage der Berliner Schneider-Innung* (Berlin, 1888), 116–17, 125–27; Hugo Rachel, *Das Berliner Wirtschaftsleben im Zeitalter des Frühkapitalismus* (Berlin, 1931), 33–37, 124–30, 163–65; and Dagmar Ladj-Teichmann, "Erziehung zur Weiblichkeit durch Textilarbeiten: Ein Beitrag zur Sozialgeschichte der Frauenbildung im 19. Jahrhundert" (Ph.D. dissertation, Technical University, Berlin, 1982), 156–57.

[4] Alice Clark, *Working Life of Women in the Seventeenth Century* (New York, 1968), 6–8. In this classic study of women's work, Clark distinguished between several different types of production, what she called domestic, family, and capitalist industry, but she did not develop these insights more fully. Also see Louise A. Tilly and Joan W. Scott, *Women, Work, and Family* (New York, 1978), 3; and Gisela Bock and Barbara Duden, "Arbeit aus Liebe—Liebe als Arbeit: Zur Entstehung der Hausarbeit im Kapitalismus," *Frauen und Wissenschaft: Beiträge zur Berliner Sommeruniversität für Frauen; Juli 1976* (Berlin, 1977), 152. Tilly and Scott discussed the difference between work for "household use" and for "exchange." In their pioneering work, Bock and Duden described categories of paid work, like sewing, that blended into housework. The focus of this important study is the origin of modern housework as women's work, a process that occurred, the authors argued, between 1880 and 1920. They discussed normative changes among Prussia's educated elite in the seventeenth and eighteenth centuries and picked up the story again at the end of the nineteenth century in North America. They omitted

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and wagework. But the labor of both was often based on the same skills. No wonder that the Naumburg tailors protested so strongly against the seventy "untrained" seamstresses who threatened the artisan ideal of skill monopoly.[5]

The root of the problem lay in the ancient conflict between urban guildsmen and the household. From its beginning, the guild system, created by towns to organize craft production, had coexisted uneasily with the household economy. Central European guilds had fought hard to separate their work from similar activity in the home and to establish distinctions between their professional output and household products. Guild prejudice exploded in the seventeenth century when manufacture for export spread to the countryside and rural households were transformed into competitive manufacturing enterprises. These households were remarkably productive, using the labor of their members in ways that undermined the guilds. Urban textile guildsmen in some areas of Central Europe articulated the issue by adopting a gender order that reserved to *guilds-men* sole control over production for exchange. By the end of the Thirty Years' War, household production had become inextricably linked with women's work in the eyes of the threatened craftsmen.[6] This association coincided with the removal of single women from independent participation in the guilds and their relegation, ideally, to service and household activities. Under the pressures of commercial capitalism, guilds saw their markets shrinking, and one response was to limit women's productive roles in goods manufacture. The fateful connection of women and the household sphere of production thereafter had a crucial effect on the experiences of women laborers in manufacturing.

Social historians of work have claimed that industrial advance was accompanied by a powerful and increasingly pervasive conception of the sexual division of labor. Wage and salary employment were defined as the proper work of men, and housework, including family maintenance and child care, was seen as the appropriate activity for women. This division originated as an ideal in the bourgeois social world and was supported by a real change resulting from industrial development: the physical separation of work and home. Historians have argued, furthermore, that with economic growth and expansion came a gradual rise in living standards, which permitted even lower-class families to approximate this work ideal at times in their domestic cycle. The division of work into two increasingly distinct sexual spheres was essential for the development of industrial capitalism in the nineteenth and early twentieth centuries.[7]

discussion of how the notions of housework and wagework became institutionalized through state policy in the course of the nineteenth century in Central Europe.

[5] Rudolf Wissell, who wrote an exhaustive three-volume study of German guildsmen and their traditions, made the important point that artisanship was essentially a matter of skill, not necessarily a matter of possession of a workshop. Wissell was a trade union secretary, a member of the German Social Democratic party in Imperial Germany, and a member of parliament during the Weimar Republic. Wissell, *Des Alten Handwerks Recht und Gewohnheit*, ed. Ernst Schraepler, 3 vols. (Berlin, 1971, 1974, 1978), 1: 8.

[6] Max Rumpf, *Deutsches Handwerker Leben und der Aufstieg der Stadt* (Stuttgart, 1955), 139. Rumpf recognized the difficulties linen craftsmen had differentiating their work from nonguild household production and from women's daily activities.

[7] Gertraude Kittler, *Hausarbeit: Zur Geschichte einer "Natur-Ressource"* (Munich, 1980); Karin Hausen,

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This article offers a broader context within which to assess the evolution of "modern" gender divisions. The focus is on the material and normative worlds of artisans and lower-class households rather than on the values generated by middle-class experience. It shows that women's work in goods manufacture was shaped profoundly by economic tension between guilds and households that erupted in the early modern period in Central Europe. Later, under state auspices, the older negative attitudes toward housework deprecated the work of women in the era of industrialization.

UNTIL INDUSTRIAL ADVANCE made domestic production redundant, household members manufactured a wide range of goods in their homes. The making of yarns, linens, upholstery, and soap was a regular part of everyday living well into the nineteenth century. These goods served household needs and were consumed on a daily basis by household members. Yet with the granting of the first guild charters in Europe in the twelfth century, the same goods were also produced for exchange—as a livelihood and profession.[8] Some guildsmen, most notably weavers, manufactured a product often indistinguishable from that made at home. In other cases, the stages of production were divided between householders and artisans. The household member, for example, cleaned and prepared horsehairs or feathers that the upholsterer then fashioned into mattresses. Similarly, tailors typically worked in the homes of their customers on coarse cloth woven by the family. In preindustrial times some products, such as bed linens and tablecloths, had not yet become artisanal and were usually made in the home.[9]

The elaborate rules and regulations that governed the intricacies of guild life were largely designed to establish guildsmen's work as a distinctive professional

"Family and Role Divisions: The Polarisation of Sexual Stereotypes of Work and Family Life," in Richard J. Evans and W. R. Lee, eds., *The German Family* (London, 1981), 51–83, and "Familie als Gegenstand historischer Sozialwissenschaft: Bemerkungen zu einer Forschungsstrategie," *Geschichte und Gesellschaft*, 1 (1975): 201; and Sibylle Meyer, "Die mühsame Arbeit des demonstrativen Müssiggangs: Über die häuslichen Pflichten der Beamtenfrauen im Kaiserreich," in Karin Hausen, ed., *Frauen suchen ihre Geschichte: Historische Studien zum 19. und 20. Jahrhundert* (Munich, 1983), 172–94. Also see, in particular, Erna Olafson Hellerstein *et al.*, eds., *Victorian Women: A Documentary Account of Women's Lives in Nineteenth-Century England, France, and the United States* (Stanford, Calif., 1981), 272–91; Tilly and Scott, *Women, Work, and Family*, 149–213; and Bock and Duden, "Arbeit aus Liebe." Tilly and Scott described a "family consumer economy" among the working classes at the end of the nineteenth century, which rested on a sexual division of labor.

[8] The guilds emerged throughout Europe between the twelfth and fifteenth centuries; thereafter, in most areas their role and influence in the urban economy declined, except in the Germanic world where they remained an important economic and sociocultural force well into the nineteenth century. Their power varied, depending on whether they controlled the urban political structure and could organize defense, taxation, and market production. Fernand Braudel, *Civilization and Capitalism, 15th–18th Century*, volume 2: *The Wheels of Commerce*, trans. Siân Reynolds (New York, 1979), 314–15; Fischer, *Handwerksrecht und Handwerkswirtschaft*, 20; and Jürgen Bergmann, *Das Berliner Handwerk in den Frühphasen der Industrialisierung* (Berlin, 1973), 22–23. Bergmann contrasted the political strength of the guilds in medieval Cologne with the weaker position of those in medieval Berlin-Cölln.

[9] *Berlin und seine Arbeit: Amtlicher Bericht der Berliner Gewerbe-Ausstellung 1896 zugleich eine Darstellung des gegenwärtigen Standes unserer gewerblichen Entwicklung* (Berlin, 1901), 225; Lothar Baar, *Die Berliner Industrie in der industriellen Revolution* (1966; reprint edn., Berlin, 1974); Gustav Schmoller, *Zur Geschichte der deutschen Kleingewerbe im 19. Jahrhundert: Statistische und nationalökonomische Untersuchungen* (Halle, 1870), 17–19; and Clark, *Working Life of Women*, 8.

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activity. Guildsmen derived their sense of status and self from jealously guarded statutes that regulated access, training, type of production, and exchange.[10] Guilds had neither the will nor the means to monopolize all social production, but in the preindustrial era they could effectively control the distribution of goods. Guildsmen monopolized sales within an established geographic area and appropriated for themselves alone the right to produce for the market. Baker guild statutes in the city of Frankfurt in 1355 established as best they could the lines between market and household production. "We have the right [to say] that no one in the city can bake unless he belongs to our guild, unless it be someone who bakes for himself and his whole household."[11] Thus, guildsmen forced a basic distinction between production for exchange (which was highly organized and regulated) and production for immediate household use (which was unregulated).

In the main, these two spheres of work coexisted independent of each other, according to explicit or implicit rules and codes. Yet the demarcation between them kept shifting, and conflict was never really absent. Central European urban chronicles are filled with examples of guildsmen in hot pursuit of "bunglers" (*Pfuscher*) and "disturbers" (*Störer*)—people who had ready access to or had acquired at home the skills to produce wares. This the guildsmen could not control. But once the itinerants tried to sell their wares in defiance of guild rules, craftsmen mobilized public authorities to run them out of town.[12] In these cases, guildsmen sought to secure their monopoly over transactions in which money was exchanged.

A criterion of esteemed work in the preindustrial era that has been largely overlooked by historians was distance from household production. Germanic society distinguished between "honorable" (*ehrlich*) and "dishonorable" labor, and the notions were of utmost importance to a world obsessed by status. They played a central role in the ranking of urban guilds and, thus, in determining who could or could not work with whom. Those labeled "dishonorable" could neither join guilds nor obtain a church position.[13] Most historians interested in the theme of

[10] Fischer, *Handwerksrecht und Handwerkswirtschaft*, 19; P. Boissonnade, *Life and Work in Medieval Europe (Fifth to Fifteenth Centuries)*, trans. Eileen Power (New York, 1927), 212–13; and Bergmann, *Das Berliner Handwerk*, 6.

[11] Rumpf, *Deutsches Handwerker Leben*, 118. The statute read, "Auch hatten wir daz recht, das nymand in der stad sulde backin, he enhette dan unssir zunfft; es enwere dan eyn man, der ime und syme gesinde wolde backin."

[12] Wissell, *Das Alten Handwerks Recht*, 1: 47; Mack Walker, *German Home Towns: Community, State, and General Estate, 1648–1871* (Ithaca, N.Y., 1971), 86–87; Rachel, *Das Berliner Wirtschaftsleben*, 13–15, 21–22; Susan C. Karant-Nunn, "Continuity and Change: Some Effects of the Reformation on the Women of Zwickau," *Sixteenth Century Journal*, 12 (1982): 26; and Rudolf Weinhold, *Töpferwerk in der Oberlausitz: Beiträge zur Geschichte des Oberlausitzer Töpferhandwerks* (Berlin, 1958), 55–56. Karant-Nunn described women selling homemade food, which angered the guilds. Also see Alfred Hoffmann, "Zur Problematik der agrarischen Nebengewerbe und der Reagrarisierung," in Hermann Kellenbenz, ed., *Agrarisches Nebengewerbe und Formen der Reagrarisierung im Spätmittelalter und 19./20. Jahrhundert* (Stuttgart, 1975), 35. Hoffmann provided information on the types of people who sold homemade goods in defiance of guild regulations.

[13] Although the words "honorable" and "dishonorable" appear in both the medieval and early modern periods, historians have argued that their meanings changed over time. In the medieval world, honor was a legal concept—the possession of rights—and honorable work was that done under license by citizens in urban areas. Dishonorable essentially applied to work done by serfs or others without specified rights, but dishonor could also be inherent in an occupation. In the early modern period, honor took on more of a moral connotation and became associated with legitimacy of birth and orderly

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work valuation have attributed both the origin and the longevity of the labels "honorable" and "dishonorable" to the urban-rural or free-unfree dichotomy that was a fundamental characteristic of social life. Some occupations were, to be sure, inherently distasteful and carried "dishonor" with them, such as hangman (*Henker*), who executed the condemned, and skinner (*Schinder*), who disposed of cattle carcasses. The opprobrium was also applied to vagabonds, gypsies, whores, tax collectors, and people associated with the theater. In the case of occupations that were not obviously repugnant, historians have argued that ultimately "dishonorable" reflected the taint of personal bondage, of birth without urban rights and privileges, which could be traced back generations.[14] "City air makes an individual free" (*Stadtluft macht frei*) was an old German saying. But I submit that "honorable" referred as well to work processes clearly dissociated from the household economy. Only such a separation could assure proper training for guildsmen and guarantee that production would take place under established procedures that constantly adjusted the ratios of masters to apprentices. Equally important was the assurance that persons who learned the trade in a household would not be admitted to guilds.

An examination of the strikingly different values placed on the making of wool and linen cloth (*Woll- und Leinengewerbe*) illustrates the negative effect of close ties to the household economy. Both trades had been highly significant in the economic expansion of Europe that began in the eleventh century. The early trading community of northwestern Europe was based primarily on the exchange of English wool and Franco-Flemish cloth. Wool cloth production expanded, and by the mid-fourteenth century the Italians had also become deeply involved in the trade. New cloth-producing centers opened up in the eastern regions of Silesia, the Lausitz, and Poland and in the western cities of Speyer and Mainz. Linen industries and the flax market also expanded, as Europeans of all social classes began to wear cotton or linen undergarments and demand lighter outer clothing. House linens, such as sheets and tablecloths, also became more common. By the thirteenth century, Constance and St. Gall had become famous for their "German" linens.[15] But, though both linen and wool weavers worked on the loom, a tool

domestic and economic behavior. After the religious wars, it seems that guardianship of morals in smaller Central European towns fell to secular institutions like the guilds. I have expanded this discussion by proposing an additional criterion. See Walker, *German Home Towns*, 91, 102–06; and Wissell, *Des Alten Handwerks Recht*, 1: 145–52, 240–54. Wissell gave numerous examples of guilds in the thirteenth and fourteenth centuries that refused to accept persons of "illegitimate" birth. For the definitions of "Ehre" and "Anrüchtigkeit," see *Meyers Grosses Konversations-Lexikon* (6th edn., Leipzig, 1908, 1909), vol. 1, and vol. 5, pp. 410, 554. Also see Heinrich Gebauer, *Die Volkswirtschaft im Königreiche Sachsen: Historisch, geographisch und statistisch dargestellt*, 3 (Dresden, 1893), 222–25; Rumpf, *Deutsches Handwerker Leben*, 68–69; and Karant-Nunn, "Continuity and Change," 24 n. 35.

[14] Walker, *German Home Towns*, 102–04; Wissell, *Des Alten Handwerks Recht*, 1: 150; Rachel, *Das Berliner Wirtschaftsleben*, 111; and H. Ermisch, "Die Anfänge der sächsischen Städtewesens," in Robert Muttke, ed., *Sächsische Volkskunde* (Leipzig, 1903), 155–56.

[15] Robert-Henri Bautier, *The Economic Development of Medieval Europe* (London, 1971), 93, 96, 166, 212–13; and Braudel, *Civilization and Capitalism, 15th–18th Century*, volume 1: *The Structures of Everyday Life: The Limits of the Possible*, trans. Siân Reynolds (London, 1979), 326–27. Also see Hans Conrad Peyer, *Leinwandgewerbe und Fernhandel des Stadt St. Gallen von den Anfängen bis 1520*, volume 1: *Quellen*; volume 2: *Übersicht, Anhang, Register* (St. Gall, 1959, 1960).

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considered among the most noble of cultural inventions, in Germanic areas only wool—not linen—cloth making was considered an honorable profession.[16]

Why linen making should have been regarded as dishonorable presents a problem, since there was nothing inherently polluting about linen's qualities as cloth.[17] In the static world of the urban guilds, however, prejudices once formed took on a life of their own. Linen weavers were shunned in part because they always had been; the sentiment had become customary. But how did the custom develop? To answer this question, we must shift our focus from the broader European terrain to the Bohemian-Saxon province of the Oberlausitz. Since the fourteenth century, the area owed its growth and prosperity to the textile trades, both wool and linen. It offers a special opportunity to assess the social valuation associated with production.

In the Oberlausitz, cloth making in general was regarded as the most honorable of work. In the city of Bautzen, cloth makers "held the oldest seal"; in Görlitz, until modern times, the neighborhoods in which wool weavers first settled were called "the craft" (*das Handwerk*); in 1356, two noble brothers not only granted the first charter to wool weavers in the village of Gersdorf in the Oberlausitz but also certified them as master artisans (*die Meister auf dem Handwerk* in the German of that time).[18] In contrast, linen makers had great difficulty being recognized as honorable. Despite repeated injunctions by the sovereign, a butcher guild in Chemnitz in the mid-fifteenth century refused to accept a linen weaver's son as an apprentice, asserting that the whole guild would have been dishonored and shunned by the city folk. There are many examples of mayors in Saxon towns who wrote on birth certificates that a child had come "from honorable parents and not from people with dubious reputations, snubbed by honorable guilds." Linen

[16] Wissell, *Des Alten Handwerks Recht*, 1: 167–72 and appendix 7. Wissell examined negative attitudes toward linen weaving, but I do not think that his analysis goes far enough.

[17] My research indicates that the negative attitudes toward linen weaving were not intrinsic to linen as a cloth; rather, guildsmen considered its mode of production insidious. In Western culture, linen cloth had been highly esteemed since biblical times (that women used linen rags—or wool for that matter—during menstruation had no negative consequences). One commentator went so far as to say, "In all ages, Linen has been considered by man as an emblem of spotless purity, and well it may be so, for what on earth is cleaner, what can be purer than fine Linen clear and white? It was the purest material in which it was possible to clothe the priesthood when performing their sacred duties, and it was used for this purpose in the idolatrous rituals of Egypt, of Greece and of Rome, as well as by the Aaronic priesthood who ministered at the altar of the God of Israel." See Alex J. Warden, *The Linen Trade, Ancient and Modern* (London, 1864), 144. Several writers also cited the judgment of Lucius Apuleius, Roman writer in the second century A.D. and author of *The Golden Ass*, who explained why men kept sacred emblems in linen, not wool, cloth. "For wool, the excretion of a sluggish body taken from sheep, was deemed a profane attire, even according to the early tenets of Orpheus and Pythagoras. But Flax, that cleanest and best production of the field, is used not only for the inner and outer clothing of the most holy priests of the Egyptians, but also for covering sacred objects." This accords with other popular notions; in the late medieval and early modern periods, shepherding was unquestionably a lowly occupation, even though the making of cloth was honored. See Warden, *Linen Trade*, 214–15; and Edgcumbe Staley, *The Guilds of Florence* (London, 1906), 350. Staley cited Apuleius, "the wise old monk of the fifth century," as the author of the above quotation, but he must have meant the classical writer.

[18] Hermann Knothe, "Geschichte des Tuchmacherhandwerks in der Oberlausitz bis Anfang des siebzehnten Jahrhunderts," *Neues Lausitzisches Magazin*, 58 (Görlitz, 1882): 246–48; Rachel, *Das Berliner Wirtschaftsleben*, 111; Rudolf Lehmann, "Niederlausitz und Oberlausitz in vergleichender geschichtlicher Betrachtung," *Jahrbuch für die Geschichte Mittel- und Ostdeutschlands*, 7 (Tübingen, 1958): 115.

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weaver was among the occupations listed as disreputable. Linen weavers in Brandenburg, Berlin, Frankfurt, Bautzen, and many other cities of the region, such as Meissen, fought these slanders in the local courts. They were aided by Saxon and Brandenburg overlords, who issued decrees placing linen weaving on an equal basis with other occupations.[19] But the laws did not change popular conceptions. Linen weavers were typically deprecated as those who "carried the ladders to the gallows." "Better skinner than linen weaver" was another popular judgment.[20] These pervasive sentiments and the resulting social ostracism lasted well into the eighteenth century.

What is essential here is the professionalization of cloth making and its transformation into an urban monopoly in the Oberlausitz. Both wool and linen weaving were part of the early manor economy, and therefore both occupations were originally "tainted" by the labor of unfree persons.[21] At the end of the thirteenth century, the territorial lords attracted cloth makers from Flanders to the area. At the time, Flanders was one of the most economically advanced regions of Europe, and it had experienced a veritable revolution in cloth making that involved a series of technological innovations (for example, the invention of the vertical loom).[22] With their technological superiority, Flemish wool weavers essentially took over the production of cloth in the urban areas of the Oberlausitz: they had the tools, established the training, and coordinated production. Oberlausitz chroniclers credited these foreigners with "inventing" the first guilds. Increasingly, each stage of production—preparing the warp, weaving, dyeing, fulling—became specialized and organized in its own guild. The guilds produced a broadcloth of higher quality than that woven in individual households.

Linen weaving, however, never lost its ties to the household economy and remained a supplemental activity in both rural and urban homes. This was partly the result of technology: the stages of linen production were not sharply differentiated, and the work tasks were fluid. Linen weavers worked with the members of their households—family and apprentices, men and women—to

[19] In 1320, Braunschweig goldsmiths refused to accept linen weavers' children (*linenweveres kindere*) as apprentices, as was true of Bremen shoemakers (1330), Hildesheim butchers (1423), and Lünen baker guilds (1601). Wissell, *Des Alten Handwerks Recht*, 1: 153, 160, 168–70, and appendix 7. In 1456 the Saxon elector officially elevated the status of linen weavers as did the later imperial decrees of 1577 and 1731. See *Meyers Grosses Konversations-Lexikon*, vol. 1, p. 554.

[20] To equate linen weavers with skinners was truly slanderous, because, of all occupations once "dishonored," only the skinner and his children who assisted him remained legally dishonored by the imperial decree of 1731. *Meyers Grosses Konversations-Lexikon*, vol. 1, p. 554. Jacob Grimm and Wilhelm Grimm noted the connection between linen weavers and the hangman's gallows. See their *Deutsches Wörterbuch*, 6 (Leipzig, 1885), 711.

[21] It should be noted that "dishonorable" was also applied to the work of Jews and Wends, and the Oberlausitz is one of the most important Wendish (Slavic) centers in German-speaking territory. But, beginning in the eleventh century, the south Oberlausitz, the area that emerged later as a protoindustrial site of linen production, was colonized by German-speakers, and the vast majority of weavers were Germans. The ethnic factor may have played a role, but I do not consider it central.

[22] P. Váczy, "La transformation de la technique et de l'organisation de l'industrie textile en Flandre aux XI^{e}–XIII^{e} siècles," *Studia Historica Academiae Scientiarum Hungaricae* (Budapest, 1960), 14; and Carlo M. Cipolla, *Before the Industrial Revolution: European Society and Economy, 1000–1700* (New York, 1976), 164. The original immigration charter for the city of Bautzen was granted in 1281. See Knothe, "Geschichte des Tuchmacherhandwerks," 241.

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produce woven goods. But the persistent view of linen weaving as part of household production was also the result of a legal compromise between noble and city interests that provided the main contours of economic development in the province. In 1345, six cities in the Oberlausitz joined together in an urban league, in its day an important and much-emulated polity. Formed originally to safeguard the trade of the cities' burghers from marauding noblemen, it quickly became a political force of its own, determined to protect the burghers' livelihood against competitive manufacturing in the countryside. Each city was granted monopolies by the kings of Bohemia, and wool making was officially established as an exclusively urban privilege. The Prague Convention of 1534 reaffirmed these separate rights and agreements between sovereign and city; it specifically provided, however, that linen weavers be allowed to weave within defined city limits as long as they did not "do business."[23] Thus, linen weaving by both men and women was permitted to continue as household work in both urban and rural areas.

In villages under noble jurisdiction (*Rittergutsdörfer*), moreover, weaving became a source of supplemental income for cottagers. Seeking to obtain a stable supply of cheap agricultural labor, the lords attracted cottagers and other small lessees to the province, which was hilly and only marginally fertile, by encouraging linen weaving during the long winter months. Weaving served multiple purposes and had the same flexibility that later characterized seamstressing—to the chagrin of both urban weavers and, later, tailor craftsmen. Linen products were turned into tribute for the lord, transformed into a marketable commodity, or produced and consumed at home. This continuing association with household economy meant that urban Oberlausitz linen guilds never established their work as distinctively urban nor particularly honorable.[24] No wonder that other guilds refused to take in a linen weaver's son or someone who married a linen weaver's daughter. Although the evidence is much sketchier for other occupations, butchers and

[23] Carl Alexander Edelmann, *Die particulare Gewerbe-Verfassung der Oberlausitz in ihrer geschichtlichen Entwicklung und Begründung* (Bautzen, 1860), 18. The relevant sections of the convention read: "Handwerks-Leute sollen in der Meile um eine jegliche der Sechsstäte nicht gehalten werden, ausserhalb denen, so über vermährte Zeit allbereit in der Meile gewohnt; aber Schmiede, die Pflug-Schaar schärffen und alt Eisen bussen, auch Leineweber, jedoch dass sie damit nicht handthieren, die mögen in der Meile bleiben." Knothe, "Geschichte des Tuchmacherhandwerks," 246; Gebauer, *Die Volkswirtschaft*, 224; and Edmund Gröllich, *Die Baumwollweberei der sächsischen Oberlausitz und ihre Entwickelung zum Grossbetrieb* (Leipzig, 1911), 8. Ladj-Teichmann described a similar pattern of development in the Prussia of Frederick I; "Erziehung zur Weiblichkeit," 65.

[24] Gebauer argued that acceptance of rural weavers into a guild dishonored it; his view has been the conventional interpretation. Speaking specifically of linen weavers, he claimed that urban magistrates attracted weavers to cities by granting them "citizenship." Guilds were required to accept citizens as members, but acceptance of weavers tainted a guild because of their tie to serf labor; *Die Volkswirtschaft*, 224. My interpretation is not necessarily incompatible with Gebauer's view, although more was at work than the issue of bondage or rural location. Such fluidity between "unfree" and "free" labor was only possible if the work tasks were similar in the household and the guilds. Also see Wissell, *Des Alten Handwerks Recht*, 1: 167–68. Knothe, in his detailed study of the history of Oberlausitz cloth makers, acknowledged that, in contrast to woolen cloth producers, the status of linen makers suffered from their association with household work, but he did not develop the point further; "Geschichte des Tuchmacherhandwerks," 246. The same is true of Gebauer's examination of the history of manufacture in the Oberlausitz; *Die Volkswirtschaft*, 223.

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bakers occasionally faced similar prejudices because their crafts were also easily performed in the household.[25]

The uneasy relationship between guilds and households turned into rivalry with the transformation of many rural households into manufacturing units for more or less distant markets. Beginning in the late sixteenth century, capitalist advances spread industry throughout the Central European countryside, and developing world markets led rural households in areas like the Oberlausitz increasingly to manufacture goods for export in return for cash income. Rural householders already had the requisite tools and skills, and they could undercut urban craftsmen. They used fully the labor of all family members—young and old, male and female—and household agricultural production permitted them to sell their goods for less than their subsistence cost.[26] Thus, the direct threat posed by these households to the livelihood of urban craftsmen increased social tensions and generated violence.

The case of urban linen weavers in the Oberlausitz furnishes a good illustration of the conflict, which worsened during the Thirty Years' War. Until 1600 the Oberlausitz urban linen guilds were able to enforce their monopolies over the market and hold at bay merchant-capitalists from Nuremberg and other South German textile cities, who tried to encourage village linen production for

[25] The place of butchers and bakers in the guild hierarchy varies too greatly over time and place to permit generalization. Several analysts of guilds in the Italian city-states of the later medieval period and of those in Paris have put forth the interpretation that I have developed. They have also claimed that a basic determinant of guild rank was the extent to which guild production was distinct from similar work done at home. Neither bakers, bread makers, nor butchers succeeded in becoming a regular part of the Six Guilds of Paris, which carried the blue canopy when the king and queen or papal legation entered the city, and members of these three guilds were rarely found in the Greater or even Intermediate Guilds of Florence. Georges Renaud wrote: "There is no need for further explanation to make us understand why the bakers and bread-makers at Florence came last on the list of twenty-one official guilds. It is useless to attribute their comparative disrepute to the supposed ease with which they could defraud their clients in the weight and quality of the bread they sold." Such ill fame came from an imperfect separation from household production. Renaud, *Guilds in the Middle Ages* (1918; reprint edn., New York, 1968), 61; and Boissonnade, *Life and Work in Medieval Europe*, 211–12. Clark made a similar point when discussing women's position in the "professional" baking and butcher trades; *Working Life of Women*, 210–19. But in the Germanic world, in medieval Bern and Berlin, for example, the same guilds were often found among the politically privileged groups. In Bern, one member from each of the smith, tanner, baker, and butcher guilds carried the city banner in battle. In thirteenth-century Berlin, four guilds (*viergewerke*) of butchers, bakers, shoemakers, and wool—but not linen—cloth makers, together with wealthy shopkeepers, comprised the second order in the political structure. On Bern, see Rumpf, *Deutsches Handwerker Leben*, 114. On Berlin, see Bergmann, *Das Berliner Handwerk*, 22–23. Butchers and bakers, however, were also subject to scorn, if folk sayings are to be believed. One popular expression said, "Die Müller sind staubig und d'Beckeknecht au, Und die Metzger sind schmutzig und die Schinderknecht au." ("Millers raise dust, and the baker's lad, too. Butchers are dirty, as is the skinner's boy.") For the quotation, see Rumpf, *Deutsches Handwerker Leben*, 131. I also think that, in German areas, butchers and bakers were among the earliest guilds regulated by local and territorial authorities because of their important role in providing good nourishment. Time and again, the authorities enforced regulations by threatening to revoke guild statutes and deregulate the sale of bread and meat, that is, open it to those not in guilds. To guildsmen, the threat was real because their skills were practiced in the household on a daily basis. Bakers and butchers were among those guildsmen who most jealously guarded their monopolies well into the early nineteenth century, as Freiherr von Stein acknowledged during the discussions in Prussia over "free trade." On the tactics of authorities, see Mieck, *Preussische Gewerbepolitik in Berlin*, 55; and Rumpf, *Deutsches Handwerker Leben*, 115–17.

[26] G. Korschelt, *Geschichte von Oderwitz* (Neu-Gersdorf, 1871), 196; and Sidney Pollard, *Peaceful Conquest: The Industrialization of Europe, 1760–1980* (Oxford, 1981), 65, 71–72.

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exchange. By the 1620s and 1630s, however, the social and economic dislocations of the war had weakened the urban guilds. South German and later British-based distributors returned to capitalize on existing skills and spread market production to the Oberlausitz countryside. In desperation, the urban guildsmen repeatedly marched on the villages, attacked them, and burned the looms. In February 1627, for example, Zittau guild-masters, town councilors, and other officers destroyed the looms and seized the yarn in the villages (*Ratsdörfer*) of Oderwitz, Olbersdorf, and Herwigsdorf; earlier they had forcibly curtailed weaving in their domain of Reichenau.[27] By the time the conflicts ended, guildsmen, as part of their denigration of the productive strengths of the rural household, had come to equate women's work with village work.[28] Sexual idiom stood for complex economic tensions—an apparently new phenomenon that warrants greater attention.

Until the mid-seventeenth century, artisanal values belittled both rural and urban household production because it was unregulated, essentially nonprofessional, and potentially competitive—that is, nonguild. In the newer controversy, when guildsmen were seeking to curtail women's participation in goods manufacture and confine their activities to the household sphere, such disesteemed work became identified as women's work. Although there was still considerable ambiguity in the mid-seventeenth century over the participants and jobs in the household economy, the increasingly closed world of urban guilds began to use gender as a fundamental determinant of work roles and their valuation.[29]

This had not always been the case. The earliest medieval guilds freely admitted women as masters. Not all guilds did so, apparently, and it may be that medieval urban manufacturing had its own equivalent of "heavy" and "light" work and segregated occupations in the manner of later industrial capitalism. But thirteenth-century Paris had eighty mixed craft guilds of men and women and fifteen female-dominated guilds for such trades as gold thread, yarn, silk, and dress manufacturing. Central European statutes explicitly mention female participants in the same century. In 1276, Augsburg regulated the wages of children—

[27] For descriptions of the conflict, see, among others, G. Korschelt, "Beiträge zur Geschichte der Webindustrie in der sächsischen Oberlausitz," *Oberlausitzer Rundshau*, vol. 11, no. 19 (1867; reprint edn., 1960), 293–94; Arno Kunze, "Vom Bauerndorf zum Weberdorf: Zur sozialen und wirtschaftlichen Struktur der Waldhufendörfer der südlichen Oberlausitz im 16., 17. und 18. Jahrhundert," in Martin Reuther, ed., *Oberlausitzer Forschungen: Beiträge zur Landesgeschichte* (Leipzig, 1961), 176–79; Ludwig Engelmann, *Geschichte von Reichenau* (Reichenau, 1904), 284–85; *Aus der Geschichte Weigsdorf: Vortrag von Karl Seiler* (Leipzig, 1904), 19; Christian Adolph Pescheck, *Handbuch der Geschichte von Zittau*, 2 (Zittau, 1837): 67; and Helmut Sieber, *Oberlausitz* (Frankfurt am Main, 1968), 22. For the unauthorized sale of linen during the unsettled, inflationary period of the Thirty Years' War, see B. O. Markgraf, "Der ländliche Gewerbezwang in Sachsen zu Beginn der Neuzeit," *Neues Archiv für sächsische Geschichte*, 30 (Dresden, 1930): 116–17.

[28] Early guild statutes (of 1500, which were reaffirmed in 1524) in the Oberlausitz province had already identified rural household work and women's work as two sources of competition. Edmund Wauer, *Geschichte der Industriedörfer Eibau und Neueibau: Eine Studie über die wirtschaftliche und kulturelle Bedeutung der südlausitzer Dörfer*, 1 (Dresden, 1913): 113–14; and Gustav Aubin, *Die Leineweberzechen in Zittau, Bautzen, und Görlitz*. Reprinted from *Abdrück aus den Jahrbüchern für Nationalökonomie und Statistik*, 104 (Jena, 1915): 608–12, 628–49. For insights into guildmen's low evaluation of both types of work, see Richard Kramer, ed., *Amtliche Katalog der Oberlausitzer Gewerbe- und Industrieausstellung Verbunden mit Ausstellung für Gartenbau und Landwirtschaft in Zittau 1902* (Zittau, n.d.), 36.

[29] Ladj-Teichmann analyzed the actual fluidity of work in the household economy; "Erziehung zur Weiblichkeit," 110.

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daughters and sons—who wanted to become apprentices; similarly, the city council of Speyer in 1360 mentioned daughters weaving cloth. Women in both Silesia and the Oberlausitz were active in cloth making. As guild statutes show, they were combers and even master-weavers (*Meisterinnen*), and the guilds were enjoined specifically "not to allow female spinners in the workshops."[30]

A policy of admitting women to a variety of nonagricultural occupations was compatible with a society that needed to use fully the productive capacities of all of its members. One thorough analyst of medieval work concluded that in the early days of the guilds "women were eligible for artisanship without restriction."[31] Although not a guild member, the wife of Bath in Chaucer's *Canterbury Tales* was by her own testimony a skillful clothier whose business was big enough to rival the Flemish cloth makers. She had numerous real-life counterparts in fourteenth-century England.[32] Female entrepreneurs like the wife of Bath promoted the big dynamic export industries that eventually undermined the guilds. Those guilds remaining in the smaller urban centers of Central Europe became increasingly exclusive, small-scale, and locally based; they were organized and run by men, even though they tolerated the membership of masters' widows. To be sure, many craftsmen relied on the labor of their wives and daughters; some even "adopted" maids as kin and paid them considerably less than apprentice wages.[33] The use of bogus family labor invariably resulted in controversy, which, however, was subsumed within guild jurisdiction. In contrast, rural family labor was not subject to guild authority and was therefore seen as more threatening. Where they could enforce their interests, guildsmen saw to it that women had few opportunities to assume independent artisan status. This exclusion of women-as-artisans coincided with the so-called decline of the guilds—a "decisive turning point," as one historian of Central Europe called it, that occurred in the mid-seventeenth century.[34] Artisanal practice identified productive work as properly male and domestic activities in households as properly female.

One impetus for the evolving artisanal ideal for the sexes, then, developed out of the economic tension between guilds and households that was an integral part of late medieval and early modern society. Numerous studies by social historians of Central Europe have employed Otto Brunner's formulation of *das "ganze Haus"* to characterize work and family life in this old society.[35] The approach stresses the

[30] Lujo Brentano, *On the History and Development of Gilds and the Origin of Trade-Unions* (London, 1870), 74; Wissell, *Des Alten Handwerks Recht*, 2: 440; Wauer, *Geschichte der Industriedörfer Eibau und Neueibau*, 114–15; and Knothe, "Geschichte des Tuchmacherhandwerks," 268–69.

[31] Wissell, *Des Alten Handwerks Recht*, 2: 439.

[32] Mary Carruthers, "The Wife of Bath and the Painting of Lions," *Proceedings of the Modern Language Association*, 94 (1979): 210.

[33] Merry Wiesner Wood, "Paltry Peddlers or Essential Merchants? Women in the Distributive Trades in Early Modern Nuremberg," *Sixteenth Century Journal*, 12 (1981): 4. For a discussion of women as independent artisans, although not in the context of Central Europe, see Natalie Zemon Davis, "Women in the Crafts in Sixteenth-Century Lyon," *Feminist Studies*, vol. 8, no. 1 (1982): 62–70.

[34] Walker, *German Home Towns*, 89.

[35] Robert Lee, "The German Family: A Critical Survey of the Current State of Historical Research," in Evans and Lee, *The German Family*, 22; Wilfried Reinighaus, "Das 'ganze Haus' und die Gesellengilden: Über die Beziehungen zwischen Meistern und Gesellen im Spätmittelalter," in Rainer S. Elkar, ed., *Deutsches Handwerk in Spätmittelalter und Früher Neuzeit* (Göttingen, 1983), 55–70; Heidi

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interconnection between production, consumption, and reproduction that was embedded in such small-scale social units as farmsteads, guilds, and workshops. Because of their focus on the unity of production and reproduction in the old society, historians have lumped together guilds and households and overlooked the tensions between the two economic entities over production and exchange. As a corollary, the productive strengths of the household and the real threat household labor presented to guild labor have not been noted.[36] Under pressure from commercial expansion that used rural household labor to a considerable extent, guildsmen began to describe the problem in gender terms. The norms they developed at this time were well expressed by Berlin tailors more than a century later. "Married women must be maintained by their husbands, know housekeeping, and care for and educate their children. The unmarried may work as domestics (for which there is no lack of opportunity locally) or engage in other feminine occupations outside regular manufacture."[37] This statement was the result of a trend underway for well over a century—the demarcation of work spheres by sex and the exclusion of women from manufacturing occupations. But cannot the statement also be seen as a normative insistence on the nature of the household as a nonproductive entity? In the guild and nonguild struggle that had already escalated by the mid-seventeenth century, a rigid definition of sex roles

Rosenbaum, *Seminar: Familie und Gesellschaftsstruktur; Materialien zu den sozioökonomischen Bedingungen von Familienformen* (Frankfurt am Main, 1978), 38; Hans Medick, "Zur strukturellen Funktion von Haushalt und Familie im Übergang von der traditionellen Agrargesellschaft zum industriellen Kapitalismus: Die protoindustrielle Familienwirtschaft," in Werner Conze, ed., *Sozialgeschichte der Familie in der Neuzeit Europas* (Stuttgart, 1976), 260–63; and Bergmann, *Das Berliner Handwerk*, 7–8.

[36] Reinighaus, however, argued specifically that guild social relations only imperfectly fit the "ganze Haus" model, but many historians of traditional society have relied on it. See Reinighaus, "Das 'ganze Haus' und die Gesellengilden," 55–56. Peter Kriedte, Hans Medick, and Jürgen Schlumbohm have been influential, empirically and theoretically, in guiding research into protoindustrial development in areas of Central Europe. They argued that the behavior of householders was rooted in the older social world that balanced production and consumption needs, and they failed to assess the ways in which involvement in manufacture altered values and preferences. They offered a comparatively static model of household production. See Kriedte *et al., Industrialisierung vor der Industrialisierung: Gewerbliche Warenproduktion auf dem Land in der Formationsperiode des Kapitalismus* (Göttingen, 1977). For a critique of the protoindustrial literature precisely along these lines, see Scott Cook, "Peasant Economy, Rural Industry, and Capitalist Development in the Oaxaca Valley, Mexico," *Journal of Peasant Studies*, 12 (1984): 12–13. Atina Grossman, in a very different context, recognized the productive potential inherent in the household, which she called a "reservoir of reproductive labor." As she indicated, the crisis of the Great Depression in Germany caused a reduction in welfare and other services. "Household tasks such as the acquisition and preparation of foodstuffs, drugs or clothing articles, which had previously been purchased readymade, were now relocated in the private home," which increased the burdens on women's time and energy. During economic crises, these homemade goods could be consumed immediately, sold, or bartered. See Grossman, "Abortion and Economic Crisis: The 1931 Campaign against Par. 218 in Germany," *New German Critique*, 14 (1978): 124. Finally, the notion of *das "ganze Haus"* has encouraged historians to focus primarily on discontinuities, as the traditional household was being transformed by industrial capitalism. But there are also important areas of continuity. In an article I coauthored with Douglas Holmes, we show that rural households in Saxony and Italy accommodated a wide variety of productive arrangements over the course of three centuries. This flexibility helped account for their survival in radically changing economic settings. See Douglas R. Holmes and Jean H. Quataert, "An Approach to Modern Labor: Worker Peasantries in Historic Saxony and the Friuli Region over Three Centuries," *Comparative Studies in Society and History* (April, 1986).

[37] For this statement, made during a court case, August 30, 1803, see Zincke, *Sechshundert Jahre Geschichte der Berliner Schneider-Gilde*, 187.

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coincided with the effort to alienate the household from the market (exchange) economy.[38]

These ideas were affirmed with growing urgency throughout the eighteenth century as many Central European states began to interfere in economic life in order to enrich their treasuries. Governments promoted cloth and tobacco exports, developed state monopolies, and acted as public entrepreneurs. Even the guilds were affected. In the wake of an imperial patent in 1731, designed explicitly to correct shortcomings in guild production, and of revised state regulations, many guilds lost autonomy. The intent of the laws was not to destroy the corporations but to infuse their operations with a new, more liberal spirit that would ease access to guild membership and ultimately increase output. The reforms were particularly extensive in Prussia, whose sovereigns were bent on strengthening central authority. There the government promulgated uniform general statutes for sixty-one trades, transferred guild supervision of training to the state, in some cases reduced the monopolistic power of guilds over sales, and generally relaxed export controls.[39]

The state gradually replaced the guilds as overseer of the labor market—a function that has become an increasingly important component of public authority. In controversies that developed over the shaping of work in the newly emerging industrial society, the state now acted as the final arbiter. This was also true in garment making. A whole new occupation was opened up for women in ready-made garment manufacture in the nineteenth century, a result of both the breakdown of tailor monopolies and a shift in consumer demand. And a new, private institution, the so-called sewing school (*Nähschule*), emerged early in the century. It offered girls instruction in sewing and provided paying jobs later on. But its presence in urban centers brought into question the role of schools and workshops in the training of labor. The guildsmen in Naumburg and other places hotly contested the right of these institutions to exist. It was the public debate over sewing schools that forced the government to crystallize its position toward production in a transitional era that redefined work in ways appropriate to a developing industrial society.

IN THE FIRST DECADES of the nineteenth century, the Prussian government consistently defended the right of women to learn sewing, despite the strenuous and repeated protests of tailor guildsmen throughout the realm. Government officials even stressed the necessity of proper and systematic training for future

[38] It would be instructive to pursue this theme by analyzing the *Hausmutterliteratur* of the early seventeenth and eighteenth centuries in Central Europe. These were sermons, almanacs, and manuals instructing wives of propertied families in their daily household routine. Their jobs included weaving, sewing, brewing, and wine making; they were also schooled in the supervision of household labor. The "products," however, were earmarked for the family's use, not for exchange. It appears that this prescriptive literature sought to divorce the productive dimensions of the household, here defined unmistakably as women's sphere, from the emerging commercial economy.

[39] Fischer, *Handwerksrecht und Handwerkswirtschaft*, 24, 29–31; Walker, *German Home Towns*, 93–100; and Rachel, *Das Berliner Wirtschaftsleben*, 18.

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housewives, who would need to sew for themselves and their families. This integral connection between training and working implicitly allowed women to transfer skills, which government officials saw as particularly appropriate for housewifery, to wagework in the marketplace. Starting in the 1840s, however, the government began to take a more active role in economic life, as the social consequences of industrial growth became more apparent: child labor, displacement of populations, and the perceived breakdown of traditional values. In this new atmosphere of government activism, Prussian ministers began to take measures based on a conception of housework as nonproductive activity. They now sought to separate wagework and housework by prescribing different training programs for those who sewed for sale and those who sewed for home use. Like the guilds, they sought to define the household apart from the exchange economy, but, also like the guilds, the state had difficulty distinguishing between spheres of work that in real life were ambiguous.

As the example of the Berlin tailors shows, tailor craftsmen believed that girls were suited for domestic service only. And the angry exchange between guild and government in the first third of the nineteenth century leaves little doubt that in smaller Central European cities, such as Naumburg, Lübben, and Zeitz, the seamstresses came from families of limited property. Lacking other opportunities, they probably would have gone into service in earlier times. But now they sought their livelihood in making what the guildsmen called "the most ordinary dresses, skirts, and coats."[40] This pattern repeated itself in Berlin in the last third of the century when urban growth and rural stagnation began to lure poor country girls to the capital in search of jobs. According to numerous inquiries by the feminist and socialist organizations, many of these young women had intended to enter service; they ended up, however, in the ready-made garment trades, preferring, it seems, the apparent freedom of manufacturing jobs at home to the constraints of domestic service.[41] Women of limited means, then, sewed for wages throughout the nineteenth century, but they dominated the apparel industry after 1870. In the formative years of the ready-made garment industry in Germany, however, a majority of the seamstresses were from middle- and lower-middle-class backgrounds. The industry's early history was bound up with the sewing school, and the clientele of the schools was middle class, although the "students" ranged from daughters of high-level officials to those of small property owners.[42]

[40] Head of the Lübben guild, September 22, 1830, ZStAM, Zivil-Kabinet, 2.2.1, Nr. 27878, Bl. 4–5; and Naumburg tailors to the Ministry of Commerce, January 22, 1822, *ibid.*, Nr. 27876, Bl. 32–37. The crisis faced by tailors in the early nineteenth century was occasioned by the official "free trade" policy and competition brought about by the transition to factory production and capitalist industry. Ironically, it was the older conception of women's work sphere as nonmarket, household production that frustrated the tailors' efforts to shut down the sewing schools.

[41] Robert Wilbrandt, *Arbeiterinnenschutz und Heimarbeit* (Jena, 1906), 8; Hans Grandke, "Berliner Kleiderkonfektion," in *Hausindustrie und Heimarbeit in Deutschland und Österreich*, volume 2: *Die Hausindustrie der Frauen in Berlin* (Leipzig, 1899), 257–58; and Gertrud Dyhrenfurth, *Die hausindustriellen Arbeiterinnen in der Berliner Blusen-, Unterrock-, Schürzen- und Tricotkonfektion* (Leipzig, 1898), 20, 25. Also see Rosmarie Beier, *Frauenarbeit und Frauenalltag im Deutschen Kaiserreich: Heimarbeiterinnen in der Berliner Bekleidungsindustrie 1880–1914* (New York, 1983), 38–39.

[42] Not much has been written about the sewing schools and their clientele. Baar, for example, argued that they were primarily for working-class girls, and Hans Grandke made a similar statement. See Baar,

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The sewing schools were private institutions, run in conjunction with the fabrication of ready-made clothing and linens. From the start, there was a logical relationship between training and wage labor, schools and workshops. The sewing schools differed from the industrial schools established by many governments in Central Europe in the late eighteenth century. The latter were located primarily in poor rural regions and were designed to improve the manual skills and productivity of the populace. The young "students" were paid wages, because their families could not do without their financial contribution. They were also taught the virtues of serious industry and discipline.[43] To mold character, particularly that of the lower classes and of women in general, was a primary objective of all state school curricula in the nineteenth century. At mid-century a defender of the sewing schools capped his case by declaring, "Through our schooling we have helped our daughters to become industrious, moral, and competent members of society."[44] The sewing school shared this moral objective but little else with the industrial schools. It was also not quite like the trade schools (*Gewerbeschulen*), established to train urban apprentices, nor the School for Female Needlework (*Schulen für weibliche Handarbeit*), an early nineteenth-century experiment of various governments in Central Europe to include hand sewing in the curriculum for primary school girls.[45] Until the 1840s, governments, particularly Prussia, were not very interested in the sewing schools. But Prussian tailors had reason to be interested, and their efforts to shut the sewing schools down forced the state to become involved. A case in Görlitz in the 1830s demonstrates the point.

Frau Nendel had received permission from Prussian officials in the late 1820s to teach girls to sew women's clothing, which she was also permitted to sell. By 1830 she had seven pupils, who had been recruited by a newspaper advertisement—a good indication that their families were middle class. Görlitz tailors were aroused. They did not argue Nendel's right to sew but challenged her involvement in both manufacturing and teaching. Women, they said, had never been allowed to set up workshops and train the next generation of workers. To these tailors and others who lodged similar protests, such schools were "illegal establishments of female tailor workshops."[46] Tailors feared them more than they did individual seamstresses because of the schools' role in vocational training. The guildsmen demanded that Nendel restrict herself either to schooling or to sewing for

Die Berliner Industrie, 80–81; and Grandke, "Die Entstehung der Berliner Wäsche-Industrie im 19. Jahrhundert," in *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*, 20 (1896): 587–607. Archival evidence, however, unmistakably reveals that patrons of the schools were middle-class girls whose families could do without their wages, at least for the period of instruction.

[43] Ladj-Teichmann, "Erziehung zur Weiblichkeit," 64, 75; Mary Jo Maynes, *Schooling for the People: Comparative Local Studies of Schooling History in France and Germany, 1750–1850* (New York, 1985), chap. 8; and Gunda Mairbäurl, *Die Familie als Werkstatt der Erziehung: Rollenbilder des Kindertheaters und soziale Realität im späten 18. Jahrhundert* (Munich, 1983), 17.

[44] E. E. Mezner to the Ministry of Interior, December 15, 1853, ZStAM, Reichministerium des Innern (hereafter, RMdI), Rep. 120 B. B. VII, 1, Nr. 4, Bd. 2, Bl. 45–57.

[45] For an excellent analysis of nineteenth-century efforts to include hand sewing in school curricula in Prussia, see Ladj-Teichmann, "Erziehung zur Weiblichkeit," 113–54. Also see Mieck, *Preussische Gewerbepolitik*, 40–42.

[46] For the protest of the Görlitz tailors, December 10, 1830, see ZStAM, Zivil-Kabinet, 2.2.1, Nr. 27878, Bl. 11–13.

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customers without assistants. But Prussian officials, who earlier had questioned whether the liberal trade policy would apply to women, now saw things differently.

In 1815 the minister of commerce had wanted to exclude women from manufacturing as long as Saxony's guilds remained intact (a prohibition that would have lasted until 1861). But, on September 24, 1822, the Prussian king promulgated by cabinet order "the right of females who were not guild members to make women's garments for pay but without assistants." This was the decision that the Naumburg tailors protested, but the government stood firm and took pride in its avowedly paternalistic policy: "The female sex in any event is much more restricted in the choice of its trade and livelihood than is the male, and women must therefore count on more governmental protection."[47]

Because of this decree, the Görlitz tailors had not opposed Nendel's right to make clothing. But they claimed that the seven girls were paid apprentices in disguise. The head of the guild could not prove it, however, since the girls were apparently sewing clothing to be worn by themselves and their families and received no money for their product. The government concluded that in this case the guildsmen were "exaggerating in the most extreme ways" their disadvantages, with the obvious intent of preventing women from learning to make garments for their own needs and thus keeping them as "captive customers" of members of the tailors' guild.[48] In handling the central complaint of practical training (*praktischer Unterricht*) and actual manufacture (*wirkliche Anfertigung*), officials linked the two intricately and rejected the guildsmen's call for one or the other. They argued that, if Nendel was not permitted to give instruction, the single girls would be deprived of an opportunity to acquire knowledge useful for their future profession of housewives. Furthermore, if a housewife sewed without benefit of instruction, that would be incompatible with the government's high objective of fostering industriousness (*Gewerbefleiss*). To separate useful instruction from the actual production of goods was impractical. The sewing school remained open.

The government's argument drew attention to the woman's "profession" as housewife. Officials acted on beliefs about proper work roles for the sexes and, in turn, helped spread and institutionalize those notions. Several normative conceptions of gender roles converged in the early nineteenth century, not only those of the artisans and craftsmen already noted but also those of the Romantics, which have recently been examined by other historians. Romantic philosophers developed a theory of "proper" gender roles, which, they presumed, corresponded to the distinct natures of the sexes, and these ideas were picked up by the educated bourgeoisie in Central Europe.[49] For both artisans and Romantics, housewife was

[47] The government's position was reproduced in the protest of the tailors; ZStAM, Zivil-Kabinet, 2.2.1, Nr. 27876, Bl. 15–22. For the letter to Chancellor Hardenberg questioning whether women were to be included in the new free trade policy, see *ibid.*, Bl. 7. For the king's order, see *ibid.*, Nr. 27878, Bl. 8. For additional information on the Saxon guilds, consult Baldwin Penndorf, *Das Innungswesen im Königreich Sachsen seit Einführung der Gewerbefreiheit* (Leipzig, 1907), ix.

[48] ZStAM, Zivil-Kabinet, 2.2.1, Nr. 27878, Bl. 8–10, February 21, 1831.

[49] Hausen, "Family and Role Divisions," 51–75. Hausen argued that in the late eighteenth century the Romantics postulated something essentially new: the anchoring of gender role in the nature or character of women. Also see Bock and Duden, "Arbeit aus Liebe," 124–25. For an interesting account

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the adult woman's true "occupation." In the 1830s officials had supported the acquisition of adequate skills by women, but, a decade later, lawmakers began to define more precisely the purposes of schooling, which were explicitly divorced from wage labor. Vocational skills acquired in schools were appropriate only for a woman's housekeeping tasks and could not be used in paid employment. The institutional separation of schools and workshops corresponded to the normative division of housework and wagework. What did such separation mean for women's work in garment making? Were the implications the same for women of different classes? Rich archival and other evidence in Berlin can help answer these questions.

Berlin in the early nineteenth century was a city of textiles. Its prosperity and renown still derived from the export of woolen, silk, half-silk, velvet, linen, and damask cloths woven with bleached or colored yarns. The production of these goods had been promoted by the government's mercantile policies, and Berlin had become one of the largest textile centers in Central Europe. In 1806, one in eight Berliners reportedly worked in textiles.[50] With industrial advance and free trade, however, cloth manufacture could not remain competitive in the city. Rents and wages were comparatively high, and there were no cheap sources of energy. Increasingly, production went out to other areas of Central Europe where costs could be kept down. But, thanks to the growing ready-made garment industry, Berlin remained a textile center. After 1870, European and North American buyers traveled to Berlin, not Paris, for fashionable women's coats, and the export trade in ready-made clothing generated five billion marks in the century's last three decades. In the mid-1890s a reliable observer estimated that one in twelve (in the season, one in eight) Berliners worked in jobs related to the apparel trade.[51]

The ready-made garment industry in Germany's new capital first developed products, such as shirts, underwear, lingerie, and sheets (*Wäschekonfektion*), that traditionally had not been monopolized by Berlin artisans. These goods were usually made in households for family consumption. Outside seamstresses, those labeled "free labor," were employed only if large quantities were needed, as for a dowry. In the early nineteenth century, linen cloth merchants began to stock the goods in response to shifts in demand from people moving alone into the city. The merchants set up sewing schools next to their shops; growing market opportunities and improved transportation networks soon stimulated production for export, first to the provinces of Prussia, then to southern Germany, and finally beyond Central Europe itself. The sewing of women's coats underwent a similar development in the 1830s, as tastes shifted to shawls and then to coats, which German retailers began to produce rather than import from Vienna and elsewhere.[52]

of how middle-class children were taught to act out appropriate norms of adult and gender behavior, see Mairbäurl, *Die Familie als Werkstatt.*

[50] Rachel, *Das Berliner Wirtschaftsleben,* 111; and Otto Wiedfeldt, *Statistische Studien zur Entwicklungsgeschichte der Berliner Industrie von 1720 bis 1890: Staats- und sozialwissenschaftliche Forschungen,* 17 (Leipzig, 1898), 68.

[51] Wiedfeldt, *Statistische Studien,* 198. Also see *Berlin und seine Arbeit,* 220; and Baar, *Die Berliner Industrie,* 73–80.

[52] Moritz Loeb, "Berliner Konfektion," in *Berliner Sittenspiegel: Der gesammelten Gross-Stadt Dokumente,*

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A new female workforce emerged at the same time that consumer preferences were shifting. Economic growth and industrial development were undercutting artisan-based industries. Simultaneously, the urban household that had combined production and consumption was beginning to break down. Urban parents could no longer use their daughters' labor in family-based production as readily as they had in earlier times. These girls needed wages and joined the labor market, and parents favored the vocational training and jobs their daughters obtained through the sewing schools. Sewing was an acceptable occupation for urban middle-class girls, for whom employment was scarce. Schooling, too, was largely unavailable for women in the early nineteenth century, despite the government's commitment to systematic instruction for all inhabitants. The sewing school offered rudimentary instruction in reading and writing in addition to vocational training. The "school" was used by some families solely for the personal advancement of their daughters. This was true of Frau Nendel's course in Görlitz. But it was the explicit connection with wage employment that gave the institution its raison d'être in the urban economy. After training, many girls went to work for the school's owner; those who became domestics had obtained a "real advantage" by learning to sew well; and wives knew they could make a little extra money for their families during hard times.[53]

In the early 1840s, when the government was beginning to take a more active role in overseeing economic life, the ambiguities between schooling and working inherent in the sewing schools' multiple functions disturbed various Prussian ministries. One of their major endeavors was to separate schooling from labor, at least for children—that is, to establish schooling as a separate activity appropriate for a stage of life for the first time recognized universally as childhood. Philippe Ariès's pioneering study shows that one effect of the "discovery of childhood" was educational reform in the eighteenth and nineteenth centuries; it was in schools that the notion of childhood as a stage of life was first institutionalized. In the early nineteenth century, the extensive use of child labor in the factories and workshops of newly emerging industries blurred the lines between child and adult—and now in socially unacceptable ways. This new sensitivity toward children coincided with a growing commitment to universal primary education, which reflected the Enlightenment's optimism and belief in progress. Since human nature was a product of the environment, reformers argued that schooling would essentially uplift and improve the lives of the new generation of workers pouring into urban areas. Besides, control over instruction was seen to counter the moral decay and decline in family life that fearful conservative rulers associated with the new working poor. The stage was set for passage of the first factory laws in Prussia in March 1839, which forbade employment of children under ten years of age in

15 (Berlin, n.d. [1906]), 7–16; Baar, *Die Berliner Industrie*, 73–82; and Beier, *Frauenarbeit und Frauenalltag*, 29–40.

[53] E. E. Mezner, the owner of the largest linen establishment in mid-century Berlin, wrote that many of his clients followed this pattern in their careers; he emphasized the variety of wagework available for those with training. ZStAM, RMdI, Rep. 120 B. B. VII, 1, Nr. 4, Bd. 2, Bl. 54–57, December 15, 1853.

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factories and mines and required persons under sixteen to present to prospective employers a school certificate showing three years of "regular" instruction.[54]

By 1842 sewing schools were a source of conflict between school officials and Prussian police, who were empowered to oversee factories. The controversy lasted two decades, and its outcome helped shape the structure of the ready-made garment industry. Officials focused first on the firm of Mezner and Sons. In 1842 the company was not only an important seller of linen cloth but also a producer of finished underclothing and shirts. It operated eight sewing-workshops for ready-made garments and in addition employed 200 seamstresses who worked at home. One of the workshops, located at Friedrichstrasse 114, came under special scrutiny. Here, 272 girls of middle-class background (*mittlerer Bürgerstand*) received free instruction in sewing from the directress. Depending on their diligence and dexterity, they had to serve a year or so in apprenticeship. This was nothing new. By Mezner's own testimony, the institution had trained about 25,000 girls since its origin in 1805. Although the number cannot be substantiated, the workshop's long existence is incontrovertible.[55] School officials initially declared the workshop a trade school and forbade instruction to those younger than fourteen, the minimum age for admission to trade schools. But most of the girls were around eleven years old, so Mezner protested the decision and insisted that his workshop was a factory. The police agreed. What tipped the scales in the eyes of the police was the actual purpose of instruction. In the cases of Mezner's and other sewing schools, apprenticeship training was not an end in itself but a means of obtaining skillful working women *for the business*. An official decision by the Ministries of Education and Finance placed the sewing workshops under the jurisdiction of the police rather than of educational authorities. For girls who had not finished their schooling, however, Mezner was required to offer twelve hours supplemental instruction per week, which he did.[56] What is obvious in the official finding is the inseparable link of schooling and wagework in the lives of young garment makers.

For over a decade, Mezner continued to have minor skirmishes with school officials, who, he charged, harassed parents and students about certificates, often refused to issue them, and threatened to fine the families. He complained that the bad publicity caused his schools to lose clients. But a new and serious element crept into the controversy after the next important factory law was passed in 1853. As factories became more clearly defined, such work became incompatible with middle-class status and sensibilities. The law of 1853 raised the minimum age for factory work to twelve years and mandated that young people under sixteen be issued a workbook containing, among other things, name, father's occupation, and school completion certificate. Furthermore, the employer had to register the

[54] Günther K. Anton, *Geschichte der preussischen Fabrikgesetzgebung bis zu ihrer Aufnahme durch die Reichsgewerbeordnung* (Leipzig, 1891), 55. Maynes devoted considerable attention to the Enlightenment influence on school reform in the nineteenth century; *Schooling for the People*, chap. 3. Also see Ariès, *Centuries of Childhood: A Social History of Family Life*, trans. Robert Baldick (New York, 1962).

[55] For Mezner's letter to the Ministry of Interior, see ZStAM, RMdI, Rep. 120 B. B. VII, 1, Nr. 4, Bd. 2, Bl. 54–57, December 1853.

[56] For the police report on Mezner's sewing schools, December 1, 1842, see ZStAM, RMdI, Rep. 120 B. B. VII, 1, Nr. 4, Bd. 1, Bl. 229–32.

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young people quarterly with the police. Everyone involved in the controversy recognized immediately that such public accountability would prove difficult to institute among middle-class girls. For the sake of appearances, women in middle-class families were supposed to be leisured. The issue became even more complicated five years later when school officials categorically decreed that factories be totally separate from schools.[57]

The school board ruling immediately affected Mezner's establishments, as officials began to count and categorize the different types of girls. One of his schools was run by the widow Decker, who had received the concession in 1824, taking it over from her mother. In 1861 her shop had ten paid aides (*Gehilfinnen*) between sixteen and twenty-three years of age, six adolescent wage-earning girls, twelve apprentices (*Lehrmädchen*) who paid no fees and received no wages but sewed on Decker's materials, and occasionally a few other students who paid a fee and worked on their own clothing.[58] Only the latter category of students was exempted unconditionally from the workbook requirement. The remaining relationships, however, were still ambiguous; students mixed with wage-earning girls as they did elsewhere. According to a detailed investigation of ready-made garment making in the early 1860s, Berlin had twenty-two sewing workshops that to all intents and purposes were "factories." Officials determined that nineteen sewing schools like Decker's mixed instruction and wage work, and they began to force a separation. The survey also counted seventy-four schools dedicated exclusively to sewing, ten of which were considered very large.[59]

In the controversy the police argued essentially for the importance of vocational training. Keeping businesses and schools under one management was "natural and necessary," since the school girls later became wage earners, particularly if they were skillful. Moreover, the police stressed that public opinion favored their arguments. But school officials won the day on a number of points. To retain schools and workshops under one directorship required official supervision by different ministries, which was impractical. Besides, it raised serious questions about the propriety of mixing children and adults, even if the young students and the more mature workers were located in different rooms. Furthermore, and here the police concurred, given the middle-class backgrounds of many of the girls and their parents' sensitivity to wagework, most workers under sixteen years of age would want to be classified as "students," exempt from the workbook requirement. The school authorities feared that somehow this subterfuge would be detrimental to actual workshops. This kind of switch was also unacceptable because school officials assigned different functions to instruction in schools and training in workshops. The functional differentiation was indeed the main argument. Only

[57] Anton, *Geschichte der preussischen Fabrikgesetzgebung*, 99; and Arthur Dodd, *Die Wirkung der Schutzbestimmungen für die jugendlichen und weiblichen Fabrikarbeiter und die Verhältnisse im Konfektionsbetriebe in Deutschland* (Jena, 1898), 2–3. Ladj-Teichmann discussed briefly the new school regulations, but with an eye to the exploitative work situation that accompanied free school instruction; "Erziehung zur Weiblichkeit," 164–67.

[58] For an audit of the sewing schools, see ZStAM, RMdI, Rep. 120, B. B. VII, 1, Nr. 4, Bd. 2, Bl. 310–12.

[59] For the school delegation report on the local sewing schools, see *ibid.*, Bl. 273–98.

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those girls learning to sew for their own personal feminine (*weibliche*) or housekeeping (*wirtschaftliche*) development were properly classified as students. Those sewing for the market were in a different category, for which workbooks were appropriate.

But an ambiguity remained. What would prevent "students" from using their training later as wage laborers? Although school officials did not pose this question directly, it bothered them, and, to answer it, they devised an institutional division to keep the two activities—work for one's own use and work for exchange—at least theoretically separate. The two institutions would teach different skills. School officials acknowledged that the workshop-factories offered much more intricate, varied, and practical training than the schools, which handled only a limited number of work processes. Workshop training, they wrote in somewhat flowery language, permitted the mastering of sewing skills in all of their "nuances" and artistic subtleties. By contrast, school instruction transmitted only the most simple tools of the trade, since parents were not interested in marketable skills for their girls. Somewhat disingenuously, officials wrote that the girls instructed in schools should be able, if need be, to find jobs despite the absence of more sophisticated training (*feineren Ausbildung*).[60]

But the industry evolved in unanticipated ways. In the first place, once the schools lost their connection to employment they lost their main economic function. In the early 1860s, as the survey cited above documents, Berlin had more than eighty-three sewing schools (when prompted by school officials, the owners of ten of the mixed schools declared them to be factories). Less than a decade later the number had dropped to thirty-three; in 1880, twenty-one; and in 1889, none. In 1872 school officials had instituted a new requirement in needlework for all primary school girls. Inclusion of such simple hand sewing was explicitly designed to enrich the school girl's housekeeping talents and prepare her to be a housewife. The curriculum excluded instruction in embroidery or other fine needlework, because these were marketable skills that could be used for wagework in the burgeoning apparel trade, which was being transformed at the time into a widespread urban home industry.[61] In practice, however, the two activities could not be kept distinct. Lower-class girls who received school instruction in hand sewing became at least superficially familiar with the trade. Even meager instruction encouraged the daughters and wives of workers to seek employment in garment making. That girls were supposed to know sewing for their family's use, however, meant that the work never lost its association with the old negative attitudes that surrounded nonmarket production.

In the second place, garment making enjoyed phenomenal growth in the last third of the nineteenth century. The industry came to depend almost exclusively on the cheap labor of working-class wives and daughters. It could not have survived in the expensive economic climate of Berlin without relying on a female

[60] For the report of December 31, 1861, to the provincial school office, see *ibid.* Also see *ibid.*, Bl. 313–21.

[61] Baar, *Die Berliner Industrie*, 83. Ladj-Teichmann provided information on the 1872 curriculum change; "Erziehung zur Weiblichkeit," 136–41, 167.

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workforce spread over thousands of workers' homes, with the resultant saving of overhead costs such as rent and heat. This particular demand for labor was readily met, but the availability of such a labor force reflected broad social changes as well. The same normative forces that had separated schools and factories in mid-century made the new industry attractive for working-class families as well. In short, what once had been class-appropriate behavior (for middle-class and lower-middle-class women) became more generally gender appropriate. As has been shown, the dominant norms defined household activities as the "proper" sphere of wives and sought to orient girls to their future activities for the home. For middle-class girls, however, sewing in schools and workshops under the guise of learning skills for their own use (even though it later became wage labor) had been tolerated. But once "workshops" were defined as "factories" and their young workers required to keep workbooks, such activity was no longer consistent with middle-class status. The same norms that held household tasks to be women's proper work coincided with working-class calculations, which also assigned family maintenance a top, although not the sole, priority for wives. This meant that wage labor outside the home became a stage in the working-class girl's life cycle. The garment industry adjusted its structure accordingly. By the last decades of the nineteenth century, those who worked in factories on collars and cuffs were typically single girls, and those who sewed for wages in their homes were married women with children. The rather easy acquisition of a sewing machine after mid-century facilitated the industry's decentralization, but technology alone cannot account for its final structure. As a recent, innovative article demonstrates, and the intense debate today over nuclear energy affirms, technology must "mesh with perceived needs" and be compatible with the specific socioeconomic climate in a given society.[62]

Archival evidence shows conclusively that after the mid-nineteenth century the new sewing machines were introduced into the workshop-factories on a rather large scale.[63] When Berlin school officials made their detailed inquiry into garment making, they described large workshops in which organized production depended on the sewing machine. By 1860 garment making resembled typical industrial work: it had become mechanized (although driven by human power) in factory buildings. The evidence provided by government inquiries contradicts the conventional interpretation, which argues that the sewing machine promoted home industry production.[64] Beginning in the late nineteenth century, middle-class reform and feminist organizations devoted considerable attention to the

[62] Dolores Greenberg, "Reassessing the Power Patterns of the Industrial Revolution: An Anglo-American Comparison," *AHR*, 87 (1982): 1254.

[63] For the school delegation report on the local sewing schools (based on a police audit of August 1859), August 27, 1861, see ZStAM, RMdI, Rep. 120, B. B. VII, 1, Nr. 4, Bd. 2, Bl. 273–98.

[64] For a classic example, see Johannes Feig, *Hausgewerbe und Fabrikbetrieb in der Berliner Wäsche-Industrie* (Leipzig, 1896), 1–3. For Feig, the sewing machine was the sole cause of decentralization, and his analysis rejects the importance of sewing workshops in the industry's evolution. Also see his second study, "Die Betriebsformen und Arbeitsverhältnisse in der Berliner Wäscheindustrie," in *Die Hausindustrie der Frauen in Berlin*, 392; Erwin Wittkowski, "Entwicklung und gegenwärtige Bedeutung der Berliner Damenkonfektion," *Zeitschrift für Handelswissenschaftliche Forschung*, 22 (1928): 289–313; and Baar, *Die Berliner Industrie*, 83.

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urban home industries, contrasting them with more traditional home work, such as rural hand weaving, and analyzing the characteristics of both types of domestic wage employment. In the main, this body of literature assumed that technology shaped the character of the new urban apparel trade. But the sewing machine was compatible with production in factories and workshops. Normative expectations about women's proper work and family responsibilities helped shape the industry's final decentralized form as much as cost factors in an expensive urban climate. Increasingly, the sewing machine, bought on time, became a familiar object in working-class homes. With it, wives and daughters took their daily labor into the market while physically staying at home to oversee family life.

In Germany, garment making became a widespread capitalist industry by the end of the nineteenth century. It flourished in urban centers like Berlin that were dominated by heavy industry. The large metal and machine trades as well as petrochemicals offered a range of jobs to men; their families, however, could not live on one income and needed the wages earned by the women in the apparel trade.[65] Wages were very low, partly because the work, as was true of women's work generally, was seen as merely a supplement to the family income. But women in garment making faced additional difficulties in securing a decent livelihood because their work was too easily identified as housework. Many of the exploitative aspects of the trade noted by contemporaries resulted from the dual uses of sewing—for one's family as well as for cash income.

Contemporary observers blamed low wages on an oversupply of labor. Job opportunities for women outside the garment industry were scarce, and the one marketable skill many girls had was sewing, learned both from their mothers and at school under the reformed curriculum. One girl spoke for most when she said, "Every girl can at least sew," while another explained her decision to get a sewing machine and set up business by saying, "I am already well versed in needlework."[66] Skill levels, however, were low. In the last third of the century, girls generally received on-the-job training lasting from fourteen weeks to six months, although there were some exceptions. Tailors, by contrast, underwent a three-year apprenticeship and could expect the highest wages in the apparel industry.[67] All efforts to institute a more rigorous, systematic apprenticeship program for women met great resistance. Obviously, the training would have raised the cost of labor

[65] Alfred Weber, "Die Entwicklungsgrundlagen der gross-städtischen Frauenhausindustrie," in *Die Hausindustrie der Frauen in Berlin*, 13–59; Karin Hausen, "Technischer Fortschritt und Frauenarbeit im 19. Jahrhundert: Zur Sozialgeschichte der Nähmaschine," *Geschichte und Gesellschaft*, 4 (1978): 148–69; and Beier, *Frauenarbeit und Frauenalltag*, 61–83.

[66] For a petition from Elisabeth Spritzenmanns to the Prussian king for money to obtain a sewing machine, see ZStAM, Zivil-Kabinet, 2.2.1, Nr. 5987, Bl. 101–02. This archive collection contains hundreds of petitions like this one from destitute women seeking public charity in order to obtain the means to work. These women described their lives in the petitions. For clippings of newspaper reports on the garment industry strike of 1896 (February 16, 1896), see ZStAM, RMdI, Rep. 120, B. B. Nr. 3, adhib. 6, Bl. 110–12.

[67] Dyhrenfurth, *Die hausindustriellen Arbeiterinnen*, 26; Marie Amelie Lipszyc, "Die Betriebsformen der Berliner Damenmassschneiderei," in *Die Hausindustrie der Frauen in Berlin*, 74–75. For a detailed report on home industry in the ready-made garment trade prepared by factory inspectors of Berlin, 1902, see German Democratic Republic, Staatsarchiv, Potsdam, Pr. Br. Rep. 30, Berlin C, Tit. 44–47, Nr. 2050, Bl. 1–35.

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in ways unacceptable to employers. Most revealing in the context of this article are the reasons behind the opposition. They were spelled out in detail when the Zittau Chamber of Commerce in 1905 rejected the decision of a district tailors guild to admit independent seamstresses as members. The chamber noted that guildsmen had undergone a regular training program, while girls (for whom there was, of course, no equivalent program) learned to make simple dresses for themselves and their families and friends and had no intention of pursuing seamstressing as an occupation. No one could equate such casual labor with the high level of skill and serious intent of craftsmen, the chamber wrote, and acceptance of female members would depress the value of the corporation.[68] Women's work in the garment industry was kept nonprofessional and low skilled in part because it was seen as just housework.

This linkage was pervasive, and in late nineteenth-century Germany it meant that even the wagework of women was not always counted as "real" labor in the eyes of the law. Germany had been the first industrializing country to adopt national pension plans for wage earners. To qualify for health, disability, or old-age insurance, workers had to be engaged in activity recognized as work by the German industrial code (*Gewerbeordnung*) of 1869. This code provided workers with such important guarantees as Sunday rest, freedom of contract, the illegality of the truck system, and continuing education for (male) youths. The growing protective legislation was extended only to those workers covered by the industrial code. Significantly, the implementing regulations did not recognize as legitimate workers women who spun, did knitting or needlework, washed or ironed in their own homes.[69] In Imperial Germany, these categories of workers were omitted from labor regulation and later denied the right to obtain old-age pensions and health insurance because the lawmakers had tied their activities to housework. In the transition to welfare capitalism, even wagework done professionally but at home along with everyday housework was not considered labor. Paul Brausenwaldt, who worked in the old-age and disability office in Stralsund, lodged a complaint with the Ministry of Interior in the mid-1890s.[70] When aged women who "spun, washed, ironed, or embroidered" for others in their own homes applied for pensions, he charged, they invariably were denied them, but when shoemakers and tailor apprentices applied—these men also worked in their homes for customers—they were granted pensions. Brausenwaldt had pinpointed one result of linking women to the household sphere.

[68] *Jahres-Bericht der Handels- und Gewerbekammer zu Zittau* (Zittau, 1905), 128.

[69] *Deutsches Reichs-Gesetzbuch für Industrie, Handel und Gewerbe*, 1 (Berlin, 1903), Gewerbeordnung. For a discussion of the relevant implementing legislation, see W. Kähler, "Materialien zur Beurteilung der rechtlichen Stellung der Hausindustrie in Deutschland," *Die Hausindustrie der Frauen in Berlin*, 2, 5, 11–12. Also see my article, "Workers' Reactions to Social Insurance: The Case of Homeweavers in the Saxon Oberlausitz in the Late Nineteenth Century," *Internationale wissenschaftliche Korrespondenz zur Geschichte der deutschen Arbeiterbewegung*, 20 (1984): 17–35.

[70] For documents on the question of extending insurance to the self-employed and small businessmen, see German Democratic Republic, Zentrales Staatsarchiv, Potsdam, RMdI, Nr. 121, Bl. 68–69.

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THIS ARTICLE HAS OFFERED a novel perspective on the history of labor divisions that have assigned women to housework (while denying its nature as work) and men to productive labor. Historians have labeled this dichotomy "modern" and see it essentially as a product of industrial development that separated the workshop from the home. These social and material changes, then, underpinned a new idea of gender roles conceived by Romantic philosophers and based on the idea of two separate spheres, each appropriate to its particular sex. Such notions were picked up first by the educated and professional elite in Central Europe and in time became the norms for many families in industrial society. My research has shown that similar ideas developed independently among urban craftsmen in the struggle over the definition of socially esteemed work and that these concepts, too, have had a long and complicated history. A key to that history in both the preindustrial and industrial eras, I have argued, is the productive strength that was inherent in the household.

The household as an economic entity subsumed within its walls different production arrangements. One arrangement served immediate needs, and its output was directly consumed by household members on a daily basis. But goods for sale were also made at home in rural and urban domestic industries. In every case production required the mastering of skills that in turn were applicable to other market settings. The productive potential inherent in households frustrated those claiming guardianship over the marketplace. Guilds were the first to face the difficulties of differentiating between market and household production. Later it was the state that could not effectively divorce the spheres of wagework and housework. These blurred lines are of particular significance in the shaping of women's work in goods production.

Guildsmen established elaborate rules and regulations to monopolize market production that distinguished between professional, organized work, on the one hand, and nonprofessional, unregulated household labor, on the other. Those activities, like linen weaving, that remained closely associated with the household economy never achieved high social value, and for centuries linen weavers suffered ostracism and abuse. In the developing world economy of the mid-seventeenth century, artisans lost control over much of the workforce. According to evidence from East Central Europe—specifically the Oberlausitz textile industry—guildsmen devalued both rural production and women's work, lumped them together, and assigned activities in the household to women, even though gender roles in household production were much more fluid and interchangeable. This development occurred at a time when Central European guilds were removing women from urban goods manufacture and were seeking, ideally, to associate wives with housekeeping and girls with domestic service. This became the artisan ideal, and it predated by at least a century the related Romantic notions of separate spheres for males and females. The rhythm of women's history does not necessarily correspond to the established dates in history that have traditionally separated one period from another. The normative association of women's work with housework predates the "modern" period, and the introduction of "free labor" in Germany

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during the early nineteenth century could not overcome the old prejudices inherent in the association.

The ambiguities between household and market production helped shape the garment-making industry, which became a major employer of women in the manufacturing sector in the nineteenth century. In the short-run, the association of women's work with housework offered seamstresses the upper hand in their conflict with Prussian guildsmen. Government officials acknowledged the importance of practical training even for personal use and kept open the early sewing schools throughout the state. But ultimately the association had negative consequences. Sewing for the family as part of housekeeping tasks was easily transformed into wage labor, but wages for sewing were low and the work kept marginally skilled because sewing was so closely associated with housework. Women's wagework could not break out of that old circle.

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Research Note

Stature and Nutrition in the Habsburg Monarchy: The Standard of Living and Economic Development in the Eighteenth Century

JOHN KOMLOS

THE RELATIONSHIP BETWEEN INDUSTRIALIZATION and the well-being of a population has been controversial since the classical economists discoursed about the issue. The debate has been particularly heated in Great Britain: the "optimists" have argued forcefully that the Industrial Revolution caused a rise in real income and an increase in the standard of living; the "pessimists," while conceding the long-term benefits of industrialization, have argued with equal fervor that at least for one or two generations it lowered workers' standard of living.[1] So conceived, the controversy is difficult to resolve,[2] not only because of the shortage of vital economic indicators but also because of myriad problems posed by the psycho-

[1] This investigation was supported by an NICHD National Research Service Award #2 T32 Hd071 from the Center for Population Research and a grant from the American Council of Learned Societies. I am indebted to Robert Fogel for awakening in me an interest in the history of human stature. I am grateful to him, Roderick Floud, and Richard Steckel for their comments on another version of this paper. I thank Roderick Floud for providing me with a computer program to estimate population height from sample height. Paul Mageli, Peter Waugh, Nona Valeriano Schlegel, Asha Narang, Erich Foltyn, Markus Hein, Klaus Erhard, Gregory Deklava, and Barbara Enger contributed invaluable service in helping to collect and process the data. I have also benefited from comments made during workshops at the following universities, institutes, and conferences: Chicago, Vienna, Linz, Salzburg, Vanderbilt, Harvard, and Northwestern; the Institute of Advanced Studies, Vienna; the 1984 Cliometrics conference at Miami University of Ohio; the Historical Institute of the Hungarian Academy of Sciences, Budapest; the Carolina Population Center, Chapel Hill, N.C.; and the Triangle Economic History Workshop in North Carolina. This project could not have been undertaken without research in Vienna, which was facilitated by the support of Gerhard Botz, Eduard März, Herbert Matis, and Erich Streisser. Computation was done at the EDV Zentrum of the Wirschaftsuniversität Wien. I thank its former director, Walter Story, for his valuable services. I am also indebted to the staff of the Kriegsarchiv, Vienna, and the staff of the Carolina Population Center for the invaluable services they rendered me and my assistants.

[1] For a negative view of the effect of the Industrial Revolution on the standard of living in Britain, see, for example, J. L. Hammond, "The Industrial Revolution and Discontent," *Economic History Review* (hereafter, *EHR*), 1st ser., 2 (1930): 215–28. For a positive view, see R. M. Hartwell, "The Rising Standard of Living in England, 1800–1850," *EHR*, 2d ser., 13 (1961): 397–416. Also see E. J. Hobsbawm, "The Standard of Living during the Industrial Revolution: A Discussion," *EHR*, 2d ser., 16 (1963): 119–34; and R. M. Hartwell, "The Standard of Living," *EHR*, 2d ser., 16 (1963): 135–46.

[2] Gertrude Himmelfarb, *The Idea of Poverty* (New York, 1983), 136.

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logical disamenities that accompany industrialization. Psychological factors could have offset the additional income that might have been generated as a result of industrialization.[3] A recent effort to address this issue uses infant mortality rates as a proxy measure of disamenities associated with urban life.[4] Even if, for the sake of simplicity, one were to disregard the psychological component of the industrializing process, there are at least two distinct questions yet to be resolved, as G. N. von Tunzelmann recently pointed out: "What actually happened to the standard of living . . . [and] what might have happened" in the absence of industrialization?[5] This study analyzes the first of these questions; the results obtained lead me to speculate on the second.

THE PESSIMISTIC VIEW GREW out of an intellectual tradition dating from Malthus and Ricardo, reinforced by the young Engels's indictment of the English middle class, which had an important role in disseminating the idea of the misery of one segment of the population to future generations.[6] An important aspect of Engels's argument was that "the poverty of the proletariat was quantitatively different from the poverty of the old poor—the preindustrial poor were less impoverished, less hardworking, less miserable."[7] This view was later shared by Marx and his

[3] P. R. G. Layard and A. A. Walters, *Microeconomic Theory* (New York, 1975), 146; and Alan Milward and S. B. Saul, *The Economic Development of Continental Europe, 1780–1870* (London, 1973), 21. For a recent controversy over various indexes of well-being, see M. W. Flinn, "English Workers' Living Standards during the Industrial Revolution: A Comment," *EHR*, 2d ser., 37 (1984): 88–92. Also see Peter Lindert and Jeffrey Williamson, "English Workers' Living Standards during the Industrial Revolution," *EHR*, 2d ser., 36 (1983): 1–25; N. F. R. Crafts, "English Workers' Real Wages during the Industrial Revolution: Some Remaining Problems," *Journal of Economic History* (hereafter, *JEH*), 45 (1985): 139–44; and L. D. Schwarz, "The Standard of Living in the Long Run: London, 1700–1860," *EHR*, 2d ser., 38 (1985): 24–41.

[4] Jeffrey G. Williamson, "Was the Industrial Revolution Worth It? Disamenities and Death in Nineteenth-Century British Towns," *Explorations in Economic History* (hereafter, *EEH*), 19 (1982): 221–45, and "Urban Disamenities, Dark Satanic Mills, and the British Standard of Living Debate," *JEH*, 44 (1981): 75–84. (The weakness in Williamson's approach lies in the assumption that the urban marginal worker was fully compensated for the greater incidence of infant mortality in cities than in rural areas.) One might note that the American experience has been less controversial than the European one. A recent regional study has shown that real hourly wages stagnated during the first two decades of the nineteenth century but then increased by two-thirds by 1860. Annual earnings increased even faster. Donald Adams, "The Standard of Living during American Industrialization: Evidence from the Brandywine Region, 1800–1860," *JEH*, 42 (1982): 903–17. Historians are in agreement that real wages fell in France during the half-century prior to the revolution. O. H. Hufton, *The Poor of Eighteenth Century France, 1750–1789* (Oxford, 1974), 16.

[5] Von Tunzelmann, "The Standard of Living Debate and Optimal Economic Growth," in J. Mokyr, ed., *The Economics of the Industrial Revolution* (New York, 1985).

[6] Friedrich Engels, *The Condition of the Working Class in England*, trans. W. O. Henderson and W. H. Chaloner (New York, 1958). See Henderson and Chaloner's introduction to the 1958 Oxford edition of Engels's book.

[7] Himmelfarb, *Idea of Poverty*, 285. Engels's eyewitness account of the misery of a segment of British workers should not be considered fully accurate or a representative sample of the total working population. He documented the suffering of the very poor and attributed their plight to the rise of the factory system only because he was ignorant of their condition before the onset of the Industrial Revolution. Children in the preindustrial era, he thought, "grew up in idyllic simplicity." Weavers "earned enough to live on" and "enjoyed a comfortable and peaceful existence." The culprit, he made clear, was "the expansion of trade" that "led to severe competition, which had an adverse effect on the standard of living of the workers." "Industry alone," he asserted time and again, "has been responsible for . . . their wretched conditions. . . . Every new machine brings with it unemployment, want, and

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followers, who, in the words of an eminent historian, "saw the pauperization of the workers as the baptismal act of capitalism."[8]

The German historian Wilhelm Abel pointed out that the Marxian contention is unacceptable because it ignores the prevalence of preindustrial rural poverty.[9] Abel devoted a whole book to describing the process of immiseration, which, with long-term fluctuations, stretched into the nineteenth century.[10] Consequently, the poverty of the working class in England and elsewhere can be better understood in a comparative perspective rather than analyzed in isolation. T. S. Ashton put it this way: "Some writers have compared the semi-skilled operatives in the new factories with the small farmers and craftsmen of an earlier generation. If comparison is to be made at all, it must be with the squatters of the countryside and the paupers of the towns, from whose ragged ranks the factory workers were largely drawn."[11] What, in other words, would have happened to the landless segment of the population and their descendants in the nineteenth century without industrialization? Bruno Hildebrand, for instance, argued that the poverty of the working class in Germany during the first half of the nineteenth century was a result of the low level of industrialization.[12] Emmanuel Le Roy Ladurie summed up his assessment of European immiseration by pointing out that it "seems to have been above all the distinctive mark of a blocked society, one incapable of raising productivity and hence the living standards of the lower classes."[13]

In order to be convinced of the intensity of rural poverty before the spread of industrialization, one might consider the case of Bohemia during the subsistence crisis of 1771. An eyewitness description of it is appalling. Sickness was prevalent because of hunger; malnutrition was endemic; the quality of clothing was poor. Children started strenuous work early in life. Peasants were uprooted, since they often had to look for work in areas where the harvest came before their own. People were dirty. Adults slept on straw, and children lay naked near the hearth. People ate grass and bread made of weed and flour dust. Some peasants worked

suffering." Engels admitted that some workers were better off than the ones he documented, but he did not attempt to write about their lives or try to ascertain or even think about income distribution among workers. Consequently, his analysis is one-sided; however, Marx accepted it and probably incorporated it into his immiseration doctrine. Engels did mention several times that industrialization increased wages, but he apparently meant nominal, not real, wages. For he insisted that the preindustrial artisan's "standard of life was much better than that of the factory worker of today [1845]." He also accepted that urban workers were better off than the rural proletariat whose lot had deteriorated even more since the beginning of the Industrial Revolution. Engels, *Condition of the Working Class in England*, xxv, xxx–4, 9–11, 24, 29, 90–92, 295. It follows from this, since in the absence of industrialization the factory workers would have joined the ranks of the rural proletariat, that the living standards of factory workers would have declined to the level of rural laborers had industrialization not proceeded as it did.

[8] Emmanuel Le Roy Ladurie, *The Territory of the Historian* (Chicago, 1979), 13.

[9] Abel, *Agrarkrisen und Agrarkonjunktur* (1935; 2d edn., Hamburg, 1966), 241.

[10] Abel, *Massenarmut und Hungerkrisen im vorindustriellen Europa* (Berlin, 1974), 14.

[11] Ashton, *An Economic History of England: The Eighteenth Century* (1955; reprint edn., London, 1972), 234.

[12] He cited the case of Oberhessen and the unindustrialized areas of Prussia; Bruno Hildebrand, *Die Nationalökonomie der Gegenwart und Zukunft* (Jena, 1922), as cited by Abel, *Agrarkrisen und Agrarkonjunktur*, 227.

[13] Le Roy Ladurie, *Territory of the Historian*, 13.

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as long as factory workers, spinning and weaving flax day and night.[14] This should not be construed to suggest that this description is any more representative of the condition of the rural population than are the sketches we have from Engels's pen of the condition of the industrial worker. Yet it does put the situation of the nineteenth-century working class in a different light: its preindustrial counterparts often did not fare better.

Scholars have concluded from the rise in cereal prices in the second half of the eighteenth century that the standard of living was falling. "We see how great a slice had been taken out of wages, and how much the purchasing power of the classes lowest in the social scale had dwindled. It was a process of impoverishment, against which they stood helpless."[15] Of course this makes sense. It is difficult to imagine that the majority of the population would have been able to protect itself against violent and unanticipated price rises. Contemporaries, too, noted that wages did not keep pace with prices.[16] Even so, the case for a deteriorating standard of living is not proved. It is difficult to draw conclusions from data on the price of grain because wage data are less reliable and scarcer than price data and daily wages are not easily converted into annual income. In addition, wage data are typically limited to a few occupations and often remained constant over decades, sometimes centuries, even in the face of great fluctuations in commodity prices. It is necessary to know if length of the workday, intensity of effort, and incidence of underemployment, unemployment, or compensations in kind varied sufficiently to offset the fluctuations in real wages obtained by deflating the wage series with the price of grain. These economic indicators are also suspect because there was considerable geographic variation in prices and rents, since some regions maintained their isolation from the world market into the nineteenth century. Initially, grain prices did not rise uniformly.[17] Furthermore, a large share of the population of the Habsburg monarchy, like those in other parts of Europe, was engaged in subsistence agriculture at the beginning of industrialization and as a result was isolated to some extent from market forces reflected in cereal prices. Finally, annual harvest data became available only during the latter third of the nineteenth century, and, consequently, estimates of the grain supply before that time are possible for only a limited number of years.[18] We can draw generalizations about the standard of living from the rise of cereal prices, but only with many caveats.

There is much evidence of the disappearance of meat from, or at least its greater scarcity in, the diet of the lower classes at the beginning of the nineteenth century.[19]

[14] Franz Mayer, "Die volkswirtschaftliche Zustände Böhmens um den Jahren 1770," *Mittheilungen des Vereins für Geschichte der Deutschen in Böhmen*, 1876, 125–49. The eyewitness pointed out the inadequate nourishment in Prachiner County by stating that the people were short; *ibid.*, 131. One might also consider the children in preindustrial Tyrol who journeyed at an early age to Germany in search of work. Otto Uhlig, *Die Schwabenkinder aus Tirol und Vorarlberg* (Innsbruck, 1978). I am indebted to Roman Sandgruber for bringing this work to my attention.

[15] B. H. Slicher Van Bath, *The Agrarian History of Western Europe, A.D. 500–1850* (London, 1963), 226.

[16] See Abel, *Agrarkrisen und Agrarkonjunktur*, 188.

[17] On the estate of Teschen in Bohemia, for instance, the price of wheat did not begin to rise steadily until 1800; Arthur Salz, *Geschichte der böhmischen Industrie in der Neuzeit* (Munich, 1913), 539.

[18] Roman Sandgruber, *Österreichische Agrarstatistik 1750–1918* (Vienna, 1978).

[19] Johann Heinrich Von Thünen theorized that, during the initial phase of economic development,

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We can quantify probable per capita meat consumption, since some data on animal stock exist, but from these estimates it is difficult to construct an index, since in the short run the rate of slaughter and animal weight could have varied dramatically and the census of the animal stock is not reliable enough for such an exercise. We can be fairly certain, however, that per capita meat consumption in Austria declined considerably, perhaps by as much as 50 percent between 1750 and 1850. The decline in meat consumption in Vienna may have started later but reached its nadir, as it did in the cities of Linz and Graz, in 1850.[20]

In sum, the problems associated with the construction of real wage series on either regional or national levels seem insurmountable despite recent noteworthy efforts.[21] This difficulty is partly a result of the poor quality or absence of data, the ambiguity of weights attached to the various component series, and the many assumptions that must be made to overcome the limitations in our knowledge. How well different segments of the population fared during these changing economic circumstances is another complicating factor. It appears that research in this area has been pushed to the limit without resolving the controversy.[22] Even if the most recent estimate of real wages in England appears plausible,[23] its independent verification by an entirely different method should be welcomed.

A new approach uses an indirect method to investigate the trend in nutrition in the eighteenth and nineteenth centuries.[24] Food clearly accounted for a large, though slowly declining, share of the European population's total expenditure. Typically, close to three-fourths of personal income was spent on alimentation in the eighteenth century.[25] Consequently, nutrition in this era is a fairly good, perhaps the best possible, indicator of the material well-being of a population. Until recently, efforts to measure systematically variations in food consumption have been frustrated by the almost complete absence of suitable data on the amount, variety, and nutritional adequacy of the food intake of populations in the past. Economic historians have therefore turned to a proxy—height—to measure nutrition.[26]

meat consumption of the poorer segments of the population would decline. See Abel, *Agrarkrisen und Agrarkonjunktur*, 238–39.

[20] Roman Sandgruber, *Die Anfänge der Konsumgesellschaft* (Vienna, 1982), 158.

[21] Jeffrey Williamson, *Did British Capitalism Breed Inequality?* (Boston, 1985).

[22] One recent article refers to this debate as having become "confused . . . arcane . . . of ever greater statistical complexity"; Roderick Floud and Kenneth Wachter, "Poverty and Physical Stature, Evidence on the Standard of Living of London Boys, 1770–1870," *Social Science History* (hereafter, *SSH*), 6 (1982): 422.

[23] Lindert and Williamson, "English Workers' Living Standards during the Industrial Revolution."

[24] Roderick Floud, "The Heights of Europeans since 1750: A New Source for European Economic History" (typescript, Department of History, University of London, 1983), 2.

[25] Fernand Braudel, *Civilization and Capitalism, 15th–18th Century*, volume 1: *The Structures of Everyday Life: The Limits of the Possible*, trans. Siân Reynolds (New York, 1981), 138. Grain alone made up approximately one-half of a man's daily food intake. In 1600, 78 percent of a bricklayer's family income was spent on food. In 1800, the share was 73 percent; Abel, *Agrarkrisen und Agrarkonjunktur*, 138.

[26] Heights were among the first data studied by social scientists. See Robert Fogel *et al.*, "Exploring the Uses of Data on Height: The Analysis of Long-Term Trends in Nutrition, Labor Welfare, and Labor Productivity," *SSH*, 6 (1982): 403, 417. An 1829 publication, for instance, stated that "human height becomes greater and growth takes place more rapidly, other things being equal, in proportion as the country is richer [and] nourishment better"; L. R. Villerme, "Mémoire sur la taille de l'homme en France," *Annales d'hygiène publique*, 1 (1829): 551–59, as cited in Floud and Wachter, "Poverty and

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During the eighteenth century, when industrialization once again accelerated, data were kept on the height of soldiers and children in various institutions. From a correlation of income, nourishment, and height, the well-being of a population can be inferred, although the relationship between the height-by-age profile of a population and the nutritional status is complex. This is not the place to go into that problem in detail. It suffices to say that in this study emphasis is placed on measuring changes in the height of a population, which genetic factors do not affect.[27]

Through natural selection, populations can adapt to different levels of nutrition over millennia, but, within the European population, nutrition has been found to be more important than genetic factors in explaining variations in human height across ethnic frontiers. Data from the Habsburg monarchy provide a good example. In the mid-eighteenth century, average heights of various ethnic groups living in different provinces were within a five-centimeter range (see Table 1). Between 1740 and 1790, with the integration of the grain market in the monarchy and the subsequent convergence of food prices, the range of the height of adult males of various nationalities decreased. By the 1790s it was about two centimeters. This is an indication that environmental factors, rather than genetic ones, were the most important determinants of the final height of the population.

The average height of children during the growing years and the terminal height of a population is, at least in market economies, a rough proxy for family income in so far as food consumption depends on real income.[28] This proxy has the advantage of measuring net nutrition (net, that is, after debilitation from disease, physical exertion, and psychological stress has been calculated). Consequently, it is perhaps a better proxy of the standard of living during the early stages of industrialization than any other measure hitherto devised.[29]

Physical Stature," 425; James M. Tanner, *A History of the Study of Human Growth* (Cambridge, 1981); Robert Fogel, "Nutrition and the Decline of Mortality since 1700: Some Preliminary Findings" (typescript, Department of Economics, University of Chicago, 1984); Roderick Floud, "The Physical State of the British Working Class, 1860–1914: Evidence from Army Recruits" (typescript, Department of History, University of London, 1984); Robert W. Fogel *et al.*, "The Economics of Mortality in North America, 1650–1910: A Description of a Research Project," *Historical Methods*, 11 (1978): 83–89, and Fogel *et al.*, "Exploring the Uses of Data on Height," 416. Weight is another indicator of nourishment, and birth weight is a good indicator of the mother's nourishment during the last trimester of pregnancy. See W. Peter Ward and Patricia C. Ward, "Infant Birth Weight and Nutrition in Industrializing Montreal," *AHR*, 89 (1984): 326.

[27] Reynaldo Martorell, "Genetics, Environment, and Growth: Issues in the Assessment of Nutritional Status," in A. Velasquez and H. Bourges, eds., *Genetic Factors in Nutrition* (New York, forthcoming), as cited in John Mellor and Bruce Johnston, "The World Food Equation: Interrelations among Development, Employment, and Food Consumption," *Journal of Economic Literature*, 22 (1984): 546. Also see Reynaldo Martorell *et al.*, "Malnutrition, Body Size, and Skeletal Maturation: Interrelationships and Implications for Catch-Up Growth," *Human Biology*, 51 (1979): 371–89; and Lawrence Greene and Francis Johnston, eds., *Social and Biological Predictors of Nutritional Status, Physical Growth, and Neurological Development* (New York, 1980).

[28] Richard H. Steckel, "Height and Per Capita Income," *Historical Methods*, 16 (1983): 1–7.

[29] Floud, "Heights of Europeans," 5; and Fogel *et al.*, "Exploring the Uses of Data on Height," 406. Using height as an index of nourishment has hitherto produced some noteworthy studies in the field of American history. According to these analyses, slaves born in the United States were better nourished than African and West Indian blacks or urban British workers but not quite as well nourished as rural white Americans of the time. Historians have also demonstrated that there were cycles in the level of nourishment in the United States, with peaks in 1795 and 1825; after 1835 there was a decline in the

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TABLE 1
Height (in Centimeters) of Soldiers
in the Habsburg Monarchy by Place of Birth[a]

Year of Birth	*Hungary*	*Lower Austria*	*Galicia*	*Bohemia*	*Moravia*
1740	170.1	n.a.	n.a.	165.3	166.3
1750	168.6	164.6	168.9	164.4	165.8
1760	167.0	n.a.	167.1	163.4	n.a.
1770	162.5	163.1	163.7	161.5	n.a.
1780	163.3	161.6	164.2	163.8	n.a.
1790	162.1	163.2	n.a.	160.8	n.a.
1800	163.0	n.a.	n.a.	n.a.	n.a.
1810	165.0	n.a.	n.a.	n.a.	n.a.
1820	163.7	n.a.	158.8	n.a.	n.a.
1830	n.a.	n.a.	160.1	n.a.	n.a.

[a] The table lists not the raw means but the estimated means after an adjustment for the minimum height requirement enforced by the army. The recruits were twenty-three years old or older, unskilled, and from rural households.

SOURCE: Vienna, Kriegsarchiv, Musterlisten and Standestabellen, Infantrieregimenter, Grundbücher.

The subsequent discussion is confined to the peasant population; data on artisans and urban dwellers await further archival research. The results reflect the standard of living of the lower classes from whose ranks army recruits were drawn. Because the army enforced a minimum height requirement for recruits, the data on the height of the adult male population must be corrected statistically. The estimation is complicated because this standard varied over time and was always flexible.[30]

ONE PATTERN THAT EMERGES from the seventy-five thousand observations extracted from the archives up to now is that stature of peasants was increasing for cohorts born in the 1740s. This feature was particularly noticeable for the sons of lower aristocrats who entered the military academy founded by Maria Theresa.[31]

general level of nutrition until about 1870. Yet soldiers during the American revolutionary war were as tall as those during the Civil War and the Second World War. Robert W. Fogel *et al.*, "Secular Changes in American and British Stature and Nutrition," *Journal of Interdisciplinary History* (hereafter, *JIH*), 14 (1983): 445–82; Gerald C. Friedman, "The Heights of Slaves in Trinidad," *SSH*, 6 (1982): 482–515; Robert Margo and Richard Steckel, "The Height of American Slaves: New Evidence on Slave Nutrition and Health," *SSH*, 6 (1982): 516–38, and "Heights of Native Born Whites during the Antebellum Period," *JEH*, 43 (1983): 167–74; Richard Steckel, "Slave Height Profiles from Coastwise Manifests," *EEH*, 16 (1979): 363–80; Fogel, "Nutrition and the Decline of Mortality"; and Floud, "Physical State of the British Working Class."

[30] I would like to thank Roderick Floud and Annabel Gregory of the University of London and Richard Steckel of Ohio State University for providing a computer program that adjusted the statistics. The method is described in detail by D. W. Wachter and James Trussell; see their "Estimating Historical Heights," *Journal of the American Statistical Association*, 77 (1982): 279–93.

[31] John Komlos, "Patterns of Children's Growth in East-Central Europe in the Eighteenth Century," *Annals of Human Biology* (forthcoming).

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This finding is extremely important because the increase in stature coincided with the favorable agricultural conditions of the period and thereby affirms the relationship between height and the nutritional status of a wide segment of the population.[32] Stature and nutritional standards were increasing during the early eighteenth century in the British colonies of North America as well, where meat appears to have become a more important element in the diet.[33]

Cross-sectional analysis shows that before the middle of the eighteenth century the tallest men in the Habsburg monarchy were born in the economically least developed lands: Hungary and Galicia (see Table 1). These economies were overwhelmingly agricultural and, in the main, not yet market oriented. Although technologically backward, the peasants were self-sufficient and lived on productive land that was not densely populated. The nutritional gradient rose from west to east: while Hungarian peasants born in the 1740s reached an average height of 170 centimeters, men in the most advanced parts of the monarchy, Lower Austria and Bohemia, were about 165 centimeters tall, the same height as British soldiers at this time. The relationship changed once the grain market in the monarchy became integrated; agricultural exports to Austria had the effect of decreasing the nutritional status of the peasant populations at the lower end of the income distribution. A hundred years later, Polish and Hungarian army recruits were among the shortest in the monarchy, having declined by about eight centimeters in stature. In 1850 the economically isolated province of Dalmatia and in the early twentieth century the predominantly agricultural province of Bosnia-Hercegovina had the tallest men in the monarchy.[34]

Another noteworthy trend is the declining height of males born in the 1750s in Bohemia and Lower Austria, a trend that resulted from population growth. Diminishing returns to labor meant diminishing consumption per capita—a typically Malthusian pattern, which undermined the well-being of the population in these provinces, even though they were industrially the most advanced of the monarchy. The resource base was not expanding at the same rate as the population grew. The declining trend of heights did not stabilize until the generation of the 1790s, some of whom matured after the end of the Napoleonic Wars and benefited from the economic advances that followed the peace.

I would not feel confident in publishing even these preliminary results were it not that other data support the notion of a decline in nutrition. The estimates in Table 1 pertain only to men over the age of twenty-three, but the pattern is similar for younger recruits. In addition, Swedish recruits, poor London boys, and American slaves suffered a similar decline toward the end of the eighteenth

[32] E. L. Jones, *Agriculture and the Industrial Revolution* (New York, 1974); and Le Roy Ladurie, *Territory of the Historian*, 13, 89, 201, 232.

[33] Kenneth Sokoloff and Georgia C. Villaflor, "The Early Achievement of Modern Stature in America," *SSH*, 6 (1982): 453–81, 470; Robert Fogel, "Growth as a Measure of the Economic Well-Being of Populations: The Eighteenth and Nineteenth Centuries," in James M. Tanner, ed., *Human Growth* (forthcoming); and Sarah McMahon, "Provisions Laid Up for the Family: Toward a History of Diet in New England, 1650–1850," *Historical Methods*, 14 (1981): 22–30.

[34] *Militär-Statistisches Jahrbuch für das Jahr 1871* (Vienna, 1873), 48.

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century.[35] The most significant corroborative evidence in support of my thesis comes from records of orphanages and army schools in the Habsburg monarchy (twenty-five thousand observations). These data have an advantage over muster-rolls. Orphanages did not enforce a minimum height requirement, and, consequently, no statistical adjustment is required to estimate mean population height from sample height. Confidence in the results is therefore increased considerably. These data show a pattern similar to that of the adult recruits: a decline in height of boys of all ages until nutrition improved in the post-Napoleonic era.[36]

Beginning in the 1750s, the Habsburg monarchs perceived this decline in the standard of living and adopted countermeasures. A subsistence crisis of immense proportions in 1770–71 and several jacqueries of an old-fashioned sort heightened their perception. Maria Theresa and her son, Joseph II, proceeded systematically to break the power of the guilds and to remove other institutional bottlenecks that stood in the way of economic development. In 1755 all persons, regardless of gender, living outside of towns were allowed to weave linen at home. In 1763 restrictions on nonguild production of cotton textiles were lifted; a similar measure for woolen textiles followed in 1764. As a result peasants could use industrial activity as a means of defending their standard of living. Then a number of agricultural reforms in the 1770s and 1780s guaranteed greater mobility to peasants and enabled them to enter the industrial labor force in greater number. The government also aided the peasantry at the expense of landlords by setting a ceiling on rents.

Several factors contributed to the success of the monarchs' economic program: the state had attained greater power, in particular, the means to intervene in the economy; it had sufficient wealth at its disposal to overcome some of the initial difficulties of industrialization; the social infrastructure as well as capital accumulation, both human and physical, had greatly expanded; and, finally, government advisers could adopt economic innovations from the West. By fostering industrialization, and thereby ameliorating the subsistence crisis, the state acted in self-defense; otherwise, its more efficient neighbors would have gained at its expense.[37]

In addition to concern for the well-being of the population, the Habsburg monarchy had another reason for taking action: the desire to prevent a crisis of ideology. A social system can function effectively only as long as its participants have so accepted established social norms that excessive monitoring of individual behavior is not required. Ideology is a substitute for monitoring. The ideology of

[35] Richard Steckel, letter to the author. Steckel was kind enough to send me a revision of the data that he and Lars G. Sandberg published; see their "Soldier, Soldier, What Made You Grow So Tall?" *Economy and History*, 23 (1980): 91–105, 98, 103. Also see Floud and Wachter, "Poverty and Physical Stature"; and Steckel, "Slave Height from Coastwise Manifests." In Leipzig a nutritional decline seems to have taken place, perhaps because of the War of the Austrian Succession. The voices of teenage boys in Leipzig changed at a later age in the 1740s than in the 1730s, indicating that a decline in nutritional status that delayed the onset of puberty began earlier there than in the Habsburg monarchy. S. F. Daw, "Age of Boy's Puberty in Leipzig, 1727–49, as Indicated by Voice Breaking in J. S. Bach's Choir Members," *Human Biology*, 42 (1970): 87–89.

[36] Komlos, "Patterns of Children's Growth."

[37] Douglass North, *Structure and Change in Economic History* (New York, 1981), 44.

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enlightened absolutism included the notion that the monarch is responsible for the well-being of his people. Therefore, a considerable decline in the standard of living of Habsburg peasants threatened the social contract. To prevent a breakdown of civil order, the monarchs opened the way for industrialization by their energetic intervention in the nation's economic life.

Questions of ideology aside, the monarchs did not welcome a decrease in their tax base. Maintenance of their standing army, and in turn their power, depended on the ability of the peasantry to pay taxes. A fiscal crisis could be expected if the standard of living of the peasantry continued to deteriorate. The institutional changes that took place in the decades after the onset of the nutritional decline can be understood only in light of the threat that this potential crisis posed to the power of absolute rulers. To maintain power, they had to impinge on the lopsided influence of the landed oligarchy, on the one hand, and the guilds, on the other, thus paving the way for economic development. That they succeeded in doing so was not only because of their perseverence and enlightened policies but also because of the external threat from Prussia that made the aristocracy more pliable than it otherwise might have been.

THE DATA FROM BOHEMIA AND LOWER AUSTRIA indicate that industrialization can be conceptualized as an antidote to the immiseration brought about by the beginning of the population explosion. That is not to say that industrialization led immediately to an improvement in living standards, but in East Central Europe, at least, it slowed the rate of deterioration until the trend could be reversed. In addition, industrialization allowed population growth to continue, even accelerate, by removing the Malthusian constraint of the declining marginal product of labor.[38] Had industrialization not proceeded as it did in these provinces, the Malthusian mechanism of diminishing returns to labor would have continued to impinge on the nutrition, and hence on the health, of the populace until population growth declined. In other words, if the industrial sector had not expanded sufficiently to provide an additional source of income, economic hardship would eventually have curtailed population growth. People could have adapted to the crisis by having fewer children, which would also have averted a demographic crisis. Without any of the above ameliorating factors, population growth would have led to the kind of disasters that occurred in earlier centuries, when population growth exceeded the growth of the resources necessary for its support. Population growth led to a decline in the standard of living, and this decline provoked institutional change, the growth of the industrial sector, and a redistribution of income that favored the peasantry over the landlords. These new economic and legal environments furthered population growth, because the industrial sector was not constrained by declining returns to labor to the same extent as the agricultural sector, but at the

[38] For a fuller account, see John Komlos and Marc Artzrouni, "A Simulation Model of Malthusian Population Dynamics in a Two-Sector Economy with Capital Accumulation between the First and Second Economic Revolutions" (typescript, Carolina Population Center, 1985).

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same time capital accumulation and technical change proceeded fast enough to absorb the growing population, thereby breaking the Malthusian cycle.[39]

Another implication of this study is that economic development can lead to a decline in nutrition for part of a society undergoing rapid economic change. This happened to a substantial segment of the Galician and Hungarian peasantry because the more advanced parts of the monarchy achieved a better and more rapid integration of the market. This finding conforms to economic theories that stress the initial disadvantages of free trade to the less developed partner.[40] Even if the whole society gains from free trade, the rapid economic change that results from it imposes hardship as well. Although the gains might be greater than the burdens, especially in the long run, the costs and benefits are not distributed evenly among various segments of the population. The less advantageously placed members of the society bear the brunt of the burdens of economic development.

A recent study of birth weights in Montreal concludes that "malnutrition and [economic] development went hand in hand."[41] The same can be said of the Habsburg monarchy's less developed regions. In economically advanced provinces, such as Bohemia and Lower Austria, the spread of malnutrition preceded the rapid industrialization of the 1760s and 1770s. Maintenance of a minimum level of nutrition in these provinces was achieved with imports of agricultural products from Hungary and Galicia. The population in the western region was able to escape the Malthusian menace only by exporting its malnutrition to the peasantry of the two agricultural provinces.

The pattern in Bohemia and Lower Austria, where the diminution in real income preceded the beginning of industrial growth, is consistent with the thesis of those economic historians who begin their analysis of the "condition-of-England-question" with "some set of exogenous changes (mainly population growth) which would have had even more severe repercussions on the standard of living without the alternative of industrial development."[42] According to Arthur J. Taylor, "The most powerful short-term influence on economic prosperity in the eighteenth century was the pressure of a rapidly expanding population."[43] François Crouzet put it this way, "The decisive factor in the undoubtedly low standards of living and widespread poverty of the working classes in the eighteenth and early nineteenth centuries was the pressure of a rising population."[44] A recent demographic study confirms the Malthusian dynamic: "Before 1800, the situation

[39] This is similar to the pattern found for eighteenth-century Flanders. "The expansion of the rural industry . . . occurred in response to the growth of population, which tended to outrun the means of subsistence. . . . The development of the cottage industry made possible the acceleration of the rate of population growth." Franklin F. Mendels, *Industrialization and Population Pressure in Eighteenth-Century Flanders* (New York, 1981), 3.

[40] For a similar pattern in contemporary Central America, see Gretel Pel and Pertti Pelto, "Diet and Delocalization: Dietary Changes since 1750," *JIH*, 14 (1983): 2, 523.

[41] Ward and Ward, "Infant Birth Weight," 326, 345.

[42] R. M. Hartwell and Stanley Engerman, "Models of Immiseration: The Theoretical Basis of Pessimism," in Arthur J. Taylor, ed., *The Standard of Living in Britain in the Industrial Revolution* (London, 1975), 213.

[43] Taylor, "Introduction," in Taylor, *Standard of Living*, lii.

[44] F. Crouzet, *Capital Formation in the Industrial Revolution* (London, 1972), 63–64.

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developed much as Malthus had insisted it must: the faster population grew, the faster the price of food rose, the lower the standard of living fell, and the grimmer the struggle to exist became. As Malthus had postulated, there were indeed long, slow oscillations in the rate of population growth and in the standard of living."[45]

In contrast, for the pessimists "the analysis generally begins with the proposition that the transition to rapid economic growth could have been achieved with less cost to the working classes."[46] That, too, is probably true. In hindsight one can argue that the government could have intervened even more energetically to better the lot of the underprivileged, but it was not prepared to respond quickly to their plight by redistributing income on a grand scale. Yet the most pressing problems of life and death were indeed solved, and in Austria they were solved without a revolution. The success of the government's economic program, given the entrenched power of the landed oligarchy, is in itself an admirable achievement.

Many contemporary observers pointed to the poverty of the hand-loom weavers as evidence of the suffering brought about by industrialization.[47] The pattern of the data on height suggests, however, that such weavers were not poor because they wove; rather, they wove because otherwise they would have been poorer. Gertrude Himmelfarb recently summed up this and related issues: "The 'standard-of-living' question . . . is generally discussed as if it were a question of industrialism alone, a measure of the social and economic costs and gains of mechanization and urbanization. In fact, . . . it was as much a product of such natural and political catastrophes as famine and war as of the industrial and agricultural revolution. It was also a product of the 'demographic revolution,' which itself was partly a consequence of industrialism and partly a cause or 'precondition' of it."[48] It was not industrialism that caused poverty; rather, the growth of the agricultural population caused poverty of sufficient magnitude to force many peasants to engage in industrial activity, even if, at first, on a part-time basis. Industrialization, in turn, enabled the labor force to emancipate itself from the Malthusian menace, which had threatened it since time immemorial.[49]

IN SUM, THE EFFECT OF INDUSTRIALIZATION on the standard of living was ameliorative in Bohemia and Lower Austria. The standard of living, as measured by height, had begun to deteriorate before the spread of industrialization, because of the growth of the agricultural labor force. The falling standard of living led the

[45] Roger Schofield, "The Impact of Scarcity and Plenty on Population Change in England, 1541–1871," *JIH*, 14 (1983): 275.

[46] Hartwell and Engerman, "Models of Immiseration," 213.

[47] An observer of hand-loom weavers in England referred to them as a race "rigidly descending to the size of Liliputians"; Floud and Wachter, "Poverty and Physical Stature," 424.

[48] Himmelfarb, *Idea of Poverty*, 135.

[49] The Malthusian positive checks (population growth constrained by the availability of the food supply) no longer operated in sixteenth-century England and thereafter, but Malthusian negative checks—restraints voluntarily imposed in response to adverse economic developments—operated until the early nineteenth century; E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1817* (Cambridge, Mass., 1981).

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government to embark on a program of institutional change that enabled many peasants in the 1750s and thereafter to enter the industrial sector, at first as cottage workers in the employ of an outputter. Protoindustrialization in Bohemia helped prevent the utter collapse of the local economy. The standard of living fell until the end of the century, then stabilized, and after a few decades began to rise again.

The extent to which this pattern of nutrition will hold up for other societies must await further research. Those who plan to explore the relationship between the standard of living and industrialization in other regions should keep in mind that it is insufficient to show a correlation between industrialization and a decline in the standard of living in order to construct a case for causation from the former process to the latter. Rather, one must explore the contemporaneous changes in the well-being of the rural population as well and determine the beginning of the downturn in the standard of living. One should furthermore investigate the rate of change of the deteriorating standard of living and correlate that with the extent of industrialization. In other words, the standard of living might be declining on a grand scale even after the onset of industrialization, but it might be declining at a slower rate than just before industrialization.

The case of the Habsburg monarchy shows how widespread rural poverty was and that immiseration of the rural population preceded the Industrial Revolution. Without industrialization, the effect of immiseration on the health of the population could have been catastrophic. By the nineteenth century, or even earlier, the population of Lower Austria and Bohemia could have been weakened to such a degree that a demographic crisis similar to the one in mid-nineteenth-century Ireland might have occurred. Indeed, in the Habsburg lands, industrialization was a conscious response designed to ameliorate the pauperization that had begun to threaten this European society.

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Reviews of Books

GENERAL

DAVID LEVINE, editor. *Proletarianization and Family History*. (Studies in Social Discontinuity.) New York: Academic. 1984. Pp. xii, 315.

This book offers a tempting sampler of recent and current directions in social, family, and demographic history. Originating at "a small, vital colloquium" held at Rutgers University in May 1983, the published collection, edited by David Levine, includes five original essays, several of which are quite substantial in length. Ranging chronologically from 1450 to 1900, the essays cover Great Britain (Levine, John R. Gillis), Western Europe more generally (Charles Tilly, Catharina Lis and Hugo Sully), and North America (Bryan D. Palmer). Taken together, they may be said to represent, especially in their emphasis on proletarianization and protoindustrialization as seminal historical processes, much of the "reigning wisdom" within the field.

With variations in focus and quality that one expects from such collections, the essays join, according to Levine, in their "concern with the social implications of the birth of capitalism and industrial society. . . . The collection itself is meant as a reflection, 30 years later, of the 'transition debate'" (p. xi). By design these studies eschew, however, traditional emphasis on economic and juridical aspects and unilinear causation for a closer focus on extended chronologies, impacts on "society in general and the family in particular," uneven and irregular processes, and multivariate "feedback mechanisms involved in the process of social change" (p. xi). Each contribution, moreover, incorporates a precis of the author's own recent research with an attempted synthesis. This feature is responsible for the excitement conveyed by several of the essays as well as the disappointment this reader experienced with others.

The essays themselves vary in how closely they adhere to the volume's stated themes. The first three, despite their often revealing differences, follow the charge the best. Tilly's "Demographic Origins of the European Proletariat" is a speculative tour de force, especially in its attempt to specify and estimate the nature and the weights of the crucial linkages that tie demographic to economic, social, geographic, familial, migratory, and related processes. To Tilly, the key component lies in "*cherchez le capitaliste*, [for] the activity of capitalists, not the abstract mechanisms of population growth, lay behind all the components of the proletariat's growth" (p. 53). This paper, which includes an excellent bibliography, demands an extended evaluation, especially of its technical estimations, that can not be provided here.

Levine's "Production, Reproduction, and the Proletarian Family in England, 1500–1851" is best read as one of a series of his writings in which these themes are pursued and through which his analysis and synthesis are refined. This essay offers a basic synthesis accessible to the novice while remaining of interest to the initiate. The links between Levine's conceptualization and arguments and those of Hans Medick and his collaborators at the Max-Planck-Institut für Geschichte, Göttingen, are important; they also call out for comparative evaluation.

In their relations to the volume's title and preface, Tilly's and Levine's chapters form the core. They are significant statements that merit wide reading and debate. Somewhat disappointing, however, is their neglect of the new challenge to the proletarianization-protoindustrialization conception and analysis. In important statements historians as divergent as Myron Gutmann and René Leboutte (*Journal of Interdisciplinary History* [1984]: 587–607), D. C. Coleman (*Economic History Review* [1983]: 435–48), and Gaye L. Gullickson (*Peasant Studies* [1982]: 106–18) have raised a number of different questions about this relatively new interpretive conception and consensus. Tilly's and Levine's responses are important to the advancement of this emergent debate.

Gillis's "Peasant, Plebian, and Proletarian Marriage in Britain, 1600–1900" previews his forthcoming *For Better, for Worse: A Political and Social History*

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of British Marriage, 1600–Present. Although this fascinating presentation of folkloric and ethnographic data and discussion of major changes in lower-class marriages—set in terms of general stages of a transition from peasants to plebians to proletarians—is certainly allied to the themes pursued by Tilly and Levine, the conceptualization and research approach are very different. Where the former depend on quantitative and demographic data and analysis, Gillis eschews them, implicitly rejecting their larger utility. A comparison of these three papers, conceptually and epistemologically, is quite revealing.

The book's final papers, constituting one-half of its pages, depart from the stated themes and focus. Although their subjects are certainly relevant, no integration is attempted. Lis and Soly's "Policing the Early Modern Proletariat, 1450–1850" is the most disappointing chapter. A summary restatement of their important *Poverty and Capitalism in Pre-Industrial Europe* (1979), the essay focuses on legal and institutional matters. Palmer's "Social Formation and Class Formation in North America, 1800–1900" is simultaneously the most narrow and the least focused essay in the book. Less interested than the others in family and demography, Palmer instead offers his own reading and synthesis of the historical relations of labor and capital in nineteenth-century Canada and the United States. In so doing, he both follows and deviates from recent emphasis in working-class history, though not always clearly. His major arguments stem from his view of the relations between "social formation" and "class formation" and on the transformations of the 1850s and 1880s.

Proletarianization and Family History is, in sum, an interesting collection that includes several valuable essays that should claim wide readership and critical attention. Neither editor nor contributor, alas, is well served by the publisher, who places a price of almost fifty dollars on a volume of just over three hundred pages. Current economics in publishing are by themselves no acceptable justification for such an action.

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HARLAN LANE. *When the Mind Hears: A History of the Deaf.* New York: Random House. 1984. Pp. xvii, 537. $24.95.

In this book Harlan Lane writes about the nineteenth century. The time period is not the book's only limitation. Lane dismisses the burden of impartial scholarship with a disclaimer that "history cannot be written without a point of view" (p. xiv). To his credit, Lane clearly states his strong bias.

The book features a supposed first-person narrative by Laurent Clerc (1785–1869), the deafened Frenchman who was a staunch advocate of the deaf community and its sign language, first in France and later in America. Lane tells the rest of the story himself in part 2 but cannot resist the patronizing statement, "I have dared to speak in Clerc's name in order to present the views of the deaf themselves as clearly and cogently as possible" (p. xvi).

At first the author appears a rational apologist for the way hearing people have treated deaf people through the centuries, but then his narrative develops villains such as Jacob Pereire in France and Alexander Melville Bell and Alexander Graham Bell (father and son) in the United States. At the opposite extreme is the apotheosis of the Abbé de l'Epée who founded Clerc's school in Paris, Thomas and Edward Gallaudet (also father and son) in the United States, and the saintly Clerc himself. One recalls Ivan Turgenev's use of the deaf Gerasim as an innocent, uncorrupted because he could not hear the evils pervading tsarist Russia.

Lane's contribution is an airing of the tyrannical influence power politics can have on the treatment of people commonly generalized into a group such as "the deaf." The nineteenth century is a useful period since electronic hearing aids were not around to muddy the generalizations with distinctions about various degrees of hearing impairment. When people who want for humanitarian reasons to teach deaf children to speak and lip-read choose to advocate that all deaf people speak and lip-read and to prohibit them from using hand signs, they become "oralists," whom Lane likens to slaveowners proscribing the native language of the group.

Alexander Graham Bell used his immense scientific prestige and fortune to campaign for oralism and assimilation of deaf people into society. He spoke out against residential schools and deaf teachers like Clerc, but Lane's moral outrage is stirred most by Bell's views on eugenics, in particular his advocacy of a national law to forbid deaf people to marry. In the author's "search for motives beneath the surface" (p. 360), he cites passages among Bell's notes that, Lane suggests, brand the scientist a racist for his Social Darwinism. Lane even hints that an oral compulsion led to Bell's inventing the telephone.

Such retrospective psychoanalysis and comparison between Clerc and Bell—"Clerc's most salient trait was humility, Bell's egoism" (p. 342)—mar the work. Lane seems not to have escaped the evil of extremes that he eschews. Nor can he escape that he is himself not one of "the deaf."

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MARGARET FORSTER. *Significant Sisters: The Grassroots of Active Feminism, 1839–1939*. New York: Alfred A. Knopf. 1985. Pp. viii, 355. $17.95.

The eight chapters of this moving book encapsulate the lives of eight women who fought for their own rights and for the rights of their sex. Four British and four American women are portrayed: Caroline Norton, Elizabeth Blackwell, Florence Nightingale, Emily Davies, Josephine Butler, Elizabeth Cady Stanton, Margaret Sanger, and Emma Goldman. Each chapter contains a largely narrative account of one woman's life and career.

Margaret Forster, who has written twelve novels and two biographies, brings considerable narrative skill to bear on her famous subjects. She focuses primarily on the close relationship between the private lives and the public works of these eight women, all of whom combined courage and tenacity with a willingness to disturb "the deep slumber of decided opinion." For the general reader or for the undergraduate coming to this field for the first time, this book is a fine introduction. Forster writes with immediacy and verve; she makes history live. And she has the biographer's virtues—sympathy for and empathy with her subjects.

Although valuable as an introduction for the nonspecialist, this book will be of limited use to professional historians. The author has drawn on many original sources but seems to be unfamiliar with much of the copious work on the history of British and American women produced by scholars during the last fifteen years, including various monographs dealing with the women on whom she has chosen to write. This leads to a number of errors. Her book is also limited by the nature of its genre: she portrays individuals rather than society, and her apparent belief that social change is brought about largely by the efforts of pioneering individuals sometimes results in oversimplification. It is simply not true that, as a result of the life of Elizabeth Blackwell, "that great fear—that trying to enter man's spheres might either be beyond woman's scope or else make her masculine in the process—was laid to rest" (p. 90). The emphasis on personal lives combined with the brevity of each chapter also results in simplisitic summaries of complex ideas. In particular, the chapter on Emma Goldman devotes many pages to a detailed and fascinating account of Goldman's marriages and affairs and their centrality to the development of her thought yet never gives a clear exposition of Goldman's anarchist views.

In general, Forster emphasizes how widely the beliefs of the early feminists differed. If there was neither a unified feminist party nor a unified feminist philosophy, it was partly because of the intellectual and personal independence of these women, most of whom fought for specific causes rather than for women's rights in general. Most of them were leaders not joiners.

The reader in search of social statistics, meticulous analyses of feminist thought, or dissections of lengthy struggles against intricate laws and deeply entrenched customs—in short, an appreciation of the complexity of legal and social change—will not be satisfied by this book. Those interested in a highly readable introduction to the lives of eight remarkable women should relish it.

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BRUCE MAZLISH. *The Meaning of Karl Marx*. New York: Oxford University Press. 1984. Pp. viii, 188. $17.95.

This is a short, extremely readable, impressionistic, and, to some extent, original book about Marx. Bruce Mazlish is thus to be congratulated: it is no mean feat, given the immense amount of literature on Marx, to have produced something that reads so freshly.

Since the rather pretentious title gives little indication of the contents, it will be helpful to summarize them for the potential reader. After an initial chapter describing Marx's thought as a secular religion and stressing the context of the Industrial Revolution, Mazlish looks at Marx's early poetry and situates him in the Romantic, anticapitalist movement of the time. The next chapters argue for a Christian influence on the formation of Marx's thought, examine his relationship to the rest of his family, and press the question of totalitarian tendencies in his thought. After this latter excursus, the book follows a more chronological framework, with extended discussions of the Paris writings, of the materialist conception of history, and of Marx's later economic writings—in particular, *Capital*. The penultimate chapter raises the question of what difference it makes to an assessment of Marx's theories to know what kind of a man he was; that is, the question of the connection between social science and biography. The final chapter reconsiders the meaning of Marxism as a secular religion.

Mazlish's treatment is more from the perspective of psychobiography or literary criticism, but he does offer a fair, if rapid, presentation of Marx's ideas. Two main theses stand out. The first is the portrayal of Marx as a religious thinker. So much is made of this that it needs more careful argumentation: it depends crucially on an equation between Marx and Marxism that Mazlish is too ready to make and on a definition of religion that is so wide as to be almost

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vacuous for analytical purposes. Looked at closely, the claim only amounts to a slightly different version of Marx's standing Hegel back on his feet. The second prominent thesis—intimately connected with the first—is the refusal to take Marx seriously as any sort of a social scientist. Far more space is devoted to discussing Marx's "pre-Marxist" writings—his early poems, his school essays, early journalism, and so on—than to what most have considered to be his main intellectual contributions: the materialist conception of history and the economic analysis of *Capital*, both of which only make their appearance in the last third of the book. This is partly owing to the intellectual style of the book: large and ill-defined notions such as science, Prometheanism, religion, and totalitarianism jostle each other in a rather jumbled melée. But it is a tribute to Mazlish's verve that someone like myself, who thinks most of the interpretations in his book highly dubious, neverthless found it consistently interesting and stimulating.

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JOHN M. HOBERMAN. *Sport and Political Ideology*. Austin: University of Texas Press. 1984. Pp. ix, 315. Cloth $24.50, paper $9.95.

This is a rich book. John M. Hoberman has done a vast amount of difficult analytical work. His contribution will be durable.

Hoberman poses a big problem and solves part of it. Sport must mean something. What? He accepts a proposition, not widely held, that he convincingly supports: that contemporary sport is a distinctive development of modern industrial society and of participatory politics. According to the author, nothing else has so captured the imagination of the masses, and sport has been useful "for virtually any ideology" (p. 1). Hoberman's survey of the significant ideologies of the twentieth century is complete. The author employs the usual terms "Right" and "Left" (the latter usually referring to Marxists). He says that every ideology ("an ideology is philosophy with ambitions" [p. 3]) has an image of the body ("the body is, in fact, an ideological variable" [p. 13]).

This is not a book on statistics, heroes, or sports as theater. It is an unflappably confident exposure of how many major (and, alas, so many minor) European intellectuals have variously employed sport. Hoberman ignores American sport, dismissing sport where it is most dynamic because it is "difficult" and "has no official status" (p. 2).

I submit my criticisms as a duty and not to demean what should become an instant classic. The writer assumes his readers will supply the background information and assessments that he should have provided. Minor figures, some of whom are resurrected by the author to provide breadth to his panorama, are not introduced, given a time frame, ranked, or evaluated to distinguish them from major literary and political figures. I was disturbed that Henning Eichberg, the major sports analyst of our age, received the same respect as Gerhard Vinnai, a clumsy polemicist. Recent West German neo-Marxist social critics have offered many original insights into modern sport as a political tranquilizer. Their French counterparts are poor by comparison. Hoberman should have at least employed irony to distinguish their value.

The book would have a yet longer shelf life if it had a better bibliography. Pagination is lacking for books and articles. Some books and articles mentioned in the text are missing from the bibliography. For some translated works (for example, that of Bero Rigauer) the original is not cited. Again, the author should have given us more of his judgments in the form of annotations.

Still, this book is a pleasure to read. Hoberman's writing is lucid; he avoids jargon and the passive voice. His frequent short quotations are apt. This is one of our generation's major works on sport.

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PHILIP D. CURTIN. *Cross-cultural Trade in World History*. (Studies in Comparative World History.) New York: Cambridge University Press. 1984. Pp. xi, 293. Cloth $34.50, paper $9.95.

This is no ordinary book. The new series sponsored by Cambridge University Press, under the daunting title Studies in Comparative World History, is off to a cracking start. Philip D. Curtin's volume is divided into eleven sections, of which the first is an introduction to the general phenomena of "trade diasporas and cross-cultural trade" and the last is an epilogue on the demise of trade diasporas in the age of global exchange and Western dominance. There is much in between. Africa (from the beginnings to the 1880s) is the subject of two chapters, and the Old World (from Sumer to Vasco da Gama) is covered in three. European and Asian traders in Far Eastern waters since 1500 each receive a section of precisely equal weight, and there is a chapter on the Armenians in Europe and the Near East and another on the fur trappers in North America.

Curtin has thus performed the laudable (and all-too-rare) feat of writing genuine "world history." His book has none of the imbalance and bias inherent in portraying European expansion as if it were the same as global history. But, unfortunately, the broader approach creates its own pitfalls. It can easily become either endless history, in which all

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human life is narrated, or episodic history, in which only selected highlights are considered. Curtin occasionally succumbs to both of these temptations. Some of his chapters (particularly those on Asia) include passages that merely chronicle long-distance migrations, and he is manifestly better informed about some areas than about others. Thus, the material concerning Africa (on which the author is, of course, an outstanding specialist) is consistently scintillating and fascinating, but that on the ancient world (in which Curtin, perhaps unwisely, tries to cover the Pochteca of pre-Columbian America as well as the Assyrians and Phoenicians) is distinctly thin. The treatment of the Orient is somewhat uneven: excellent on the Bugis and Banians, but disappointing on Malacca and Macao. Despite this, Curtin succeeds in keeping to his theme, whatever the times and places his text considers.

What, then, does this book tell us that is new? First, several other theories concerning the history of international commerce are subjected to critical revision: Frederick C. Lane's ideas on the "hidden protection costs" associated with long-distance trade are successfully applied to Africa (pp. 41–42); Karl Polanyi's "elaborate discussion" of West African port commerce as an example of "marketless trade" is discredited (p. 58 n. 32). Beyond Africa, Curtin is more cautious—he does not discuss, for example, the merits and limits of the broad theories that underpin Joseph Needham's *Science and Civilization in China*, which has much to say about cross-cultural contacts—and he is always careful to show his readers the extent to which each argument is based on an impressive range of up-to-date sources. At one point Curtin notes, almost with regret, that "well over half of the most valuable authorities I consulted were published after the early 1970s" (p. x), yet this modernity contributes to one of the book's major strengths, since older works are often marred by an absurd Eurocentric bias. A second strength is, of course, the extraordinary wealth of diverse information presented; few scholars (if any) will be familiar with all the areas and phenomena surveyed here. Indeed, perhaps the chief virtue of this book is precisely its relentlessly comparative approach, which constantly juxtaposes what readers may know about one region or topic with parallels or paradoxes that they will not know about another. This admirable study has something new and interesting for everyone: it is a triumph.

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JÖRG FISCH. *Die europäische Expansion und das Völkerrecht: Die Auseinandersetzungen um den Status der überseeischen Gebiete vom 15. Jahrhundert bis zur Gegenwart.* (Beiträge zur Kolonial- und Überseegeschichte, number 26.) Stuttgart: Steiner. 1984. Pp. xvi, 569. DM 82.

Most historians will probably be somewhat puzzled by this book. Although it ostensibly deals with a historical subject, it is primarily a treatise on international law in historical context, not a conventional history. The book contains much of value to historians, but it responds primarily to questions arising from the traditions of legal scholarship, not to the concerns of most historians for causal analysis or social and cultural description.

Jörg Fisch's aim is to describe the emergence of a universal system of international law as a result of European expansion since the fifteenth century. He argues that this system was developed in Europe and imposed on the rest of the world by the most egregious violation of the principles of rational international law: imperialism. The collapse of the European colonial empires in modern times left the system functioning on a worldwide basis. The universalization of international law was thus one of the most important results of European expansion. Few will, I suspect, be inclined to dispute this thesis, although many will consider Fisch's presentation excessively Eurocentric and his analysis of causes rather narrow.

The book is organized around abstract descriptive categories of international legal relations developed in the first theoretical section. This sometimes makes the historical part of the author's argument difficult to follow, since the discussion of change tends to be fragmented. The weakest part of the book is its first substantive chapter on relations between European states and non-European polities since the fifteenth century. These he dismisses as unimportant to the development of international law because, first, whatever the initial relationship between European and non-European states, the ultimate result was European hegemony and, second, even in the most "advanced" parts of the extra-European world, real systems of international law were absent. Relationships among Asian states before European penetration are described as "feudal," which presumably means that they were envisioned as personal connections of dominance and subordination between rulers rather than legal relations among sovereign equals. This is far too simple; it ignores a great deal of interesting research on the complexities of European-Asian interaction during the first centuries of European expansion. The idea that universal international law was a European imposition is also difficult to accept fully, since the author does not actually demonstrate that extra-European systems of international relations did not involve concepts of international law and did not affect later legal development.

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The second and third chapters discuss the changing positions of the major European states on the legal status of their own and other countries' colonies. These positions and their convergence to form an accepted canon of international law for overseas application are nicely summarized. The author appears to be concerned mainly with countering the view that the idea of "no peace beyond the line"—the belief that international law governing the relations among European states did not apply overseas—was ever an accepted part of the practice and theory of international relations. I think he proves his point, but probably at much greater length than is necessary.

The later chapters summarize the development in Europe of legal theory concerning the non-European world and the emergence of universal international law in treaties and arbitration cases from colonial times down to the era of decolonization. These chapters are useful compendia, but they tend to ignore the larger economic, social, and even political contexts in which the changes they describe took place.

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JACQUES MARSEILLE. *Empire colonial et capitalisme français: Histoire d'un divorce.* (L'Aventure Humaine.) Paris: Albin Michel. 1984. Pp. 461. 130 fr.

Did empire pay? No question about modern European overseas expansion has generated more historical debate than this. Among the answers, of course, figures Lenin's significant tract *Imperialism, the Highest Stage of Capitalism* to which Jacques Marseille's *Empire colonial et capitalisme français* is a most forceful dissenting response, but one that emphasizes the economics of empire in a different and remarkable way.

Marseille's argument has three major elements. First, he maintains that empire was profitable, but only to those sectors of the economy that were no longer competitive. Second, industrial capital, not financial capital, encouraged and dominated the *mise en valeur* of the empire. Third, and consequently, the French colonial empire retarded the modernization of French industry.

Marseille's closely argued analysis is effectively supported by statistical data that do not remain in an unrefined global form but rather are separated into distinct parts to demonstrate just who profits from empire and why. Moreover, Marseille is also interested in what he terms "social commerce"—which social groups and classes purchased the imported goods—for without understanding this "social commerce," he contends, "global commerce" cannot be appreciated in then-contemporary terms.

The most telling part of the book's thesis is the one that deals with the colonies as markets, the most persistent of the economic arguments in modern French colonial history. Marseille offers a trenchant conclusion: "The colonial market was primarily seized by declining branches of commerce and relatively ignored by those that were growing" (p. 85). Empire was therefore a special place in the sun, a sort of retirement home for aged and no longer competitive French industries. There, candle manufacturers, cotton goods producers, and brewers, for example, were able to sell products that could not compete effectively in the world market.

The role of the state in this phenomenon is described not as collaborative or collusive but as confusedly directive. Marseille sees, as most students of the subject have, a colonial administration fraught with disorganization—no forceful central authority, insufficient staff, and a lack of close communication between the ministry in Paris and the highly automous governors-general overseas. The one metaphor the author frequently uses to describe the role of the state seems the most appropriate: it was a crutch on which weak commerce could support itself.

If there was one moment when the economic problems of the French empire were brought into sharp focus, it was at the time of the Great Depression and, specifically, the year 1930. Remarking that the history of the depression and its relationship to colonial empire still needs to be written, Marseille makes a major contribution himself. He discusses the three major lines of argument offered as a solution to France's economic ills by those individuals viewing the world from a colonial perspective: autarchy, liberalism, and a form of development stressing the industrialization of Indochina and North Africa.

Marseille has done what the French higher degree program is designed to do; he has offered a major work of scholarship that refines our knowledge of a significant historical problem. All that is now needed is the wisdom of an American university press to bring out an English edition of this important book so that all students of imperialism, whatever their national interests, can add Marseille's well-wrought arguments to their own evaluation of this perplexing and compelling subject.

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VIVIAN VALE. *The American Peril: Challenge to Britain on the North Atlantic, 1901–1904.* Dover, N.H.: Manchester University Press. 1984. Pp. viii, 256. $27.00.

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At the turn of the nineteenth century, exports from the United States to Europe were increasing to such an extent as to raise apprehension and even alarm in the Old World. At times, the flood of goods from across the Atlantic seemed no less than a devastating American invasion, and, together with the "yellow peril," an "American peril" haunted some minds not altogether foolish. Although a highly significant rapprochement between Great Britain and the United States was taking place at about this same time, the rising American economic colossus was a frightening specter to many British businessmen, government officials, and publicists.

All this is well known. In this book Vivian Vale confines his attention to Great Britain and the United States, with a side look at Germany. In his opening chapter, an overview of the American economic invasion of Britain, he is less concerned with American exports in general than with the British concern over the proliferating American trusts and their designs on foreign markets. "And now, it seemed, these omnivorous creatures, the trusts, having exhausted the forage of their native habitat, were emerging to raven on European pastures also" (p. 17).

This is background for the main subject of the book, namely, one particular American trust and the British response to it—a matter far less familiar than that of burgeoning American exports. The trust was the International Mercantile Marine Company (IMM), a combine that included many of the principal British and American North Atlantic shipping lines and also had an important German component. This gigantic international combine—sponsored by no less a figure than the redoubtable J. P. Morgan, then at the height of his renown—seemed to the British public and government so formidable a menace to Britain's commercial supremacy, long taken for granted, that London felt compelled to take vigorous countermeasures, almost as though it were confronting a powerful enemy nation. These took the form, first, of a governmental agreement with the combine providing that British companies in the IMM remain British and (to compensate Morgan) be treated equally with other British companies as regarded military and postal subsidies. The second response was a contract between the government and the Cunard line (the major British shipping company not in the IMM) that was intended—and in the event successfully so—to bolster Cunard's resistance to Morgan's enticements.

Vale's account of these events is straightforward, well written, exhaustively researched, and full of detail. One may wonder whether the subject is sufficiently important to warrant so much attention, but anyone wishing to learn about the stir created by Morgan's combine can do no better than turn to this book. For a thorough study of a small topic it could scarcely be improved on.

Vale tries, in a short epilogue, to put these matters into a larger perspective, and here there is more room for controversy. He contends, for example, that the alarm generated in Great Britain by the IMM and its German element "ensured, as never before, that the British public were aware in some detail of the nature of the German threat" (p. 228)—although it should be noted that the alarm did nothing to disturb friendship with the United States. For another, the agitation helped ensure that "free trade never again enjoyed the same popularity with the British public" (p. 228). These are large claims for what Vale proceeds to argue was a transatlantic threat of little substance that diminished as suddenly as it had appeared. By the late 1900s Morgan was "a tyro, not a tycoon," in shipping affairs (p. 236), and one can only wonder why the mighty British empire, its overseas commerce unassailably dominant, was "so deeply perturbed by a shipping threat of relatively minute dimensions" (p. 238). Indeed, "the American peril" and the "challenge to Britain on the North Atlantic" seem to have been neither perilous nor challenging.

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LLOYD C. GARDNER. *Safe for Democracy: The Anglo-American Response to Revolution, 1913–1923*. New York: Oxford University Press. 1984. Pp. xii, 383. $25.00.

Since 1976 Lloyd C. Gardner has published several studies on Woodrow Wilson's response to revolution in Mexico and Russia. In *Safe for Democracy* Gardner retains his perspective on Wilson but significantly expands his scope to include the Anglo-American dialogue from 1913 through 1923 on these revolutions and also on the Chinese Revolution. The result is a fresh reworking of several topics that produces new insights on Wilson, the British-American dialogue, and the transition from late Wilsonian diplomacy to Republican foreign policy.

Gardner approaches his subject from the top down, examining the interplay of ideas and policies between British and American policy makers. From the response to the first rumblings in Mexico through the final maneuvers on China at the Washington Conference and David Lloyd George's negotiations with the Soviet Union in 1923, Gardner keeps the focus on the memoranda and letters exchanged by key statesmen on both sides of the Atlantic. Only occasionally does he take into account other considerations, such as American oil interest in Mexico, bankers' interests in China, and domestic political restraints on Lloyd George.

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Gardner's view of Wilson is within the general framework of the "New Diplomacy" developed by N. Gordon Levin and others. Gardner, however, devotes more attention than Levin to the subtleties and nuances in Wilson's ideas as he struggled to assert liberalism versus the imperialism of the "Old Diplomacy" and the revolutionary ideas of "watchful waiting," trying to support liberal forces and the principle of self-determination.

Wilson's main companion on this quest was Lloyd George, although the British leader, as Gardner admits, was not directly involved with Mexico or China. Nor did Wilson have sustained involvement with China; it was Secretary of State Robert Lansing who maneuvered with Japan to reduce European spheres of influence and help liberate China. Sir Edward Grey, the British foreign minister, directed the resistance to Washington's stance toward China and Mexico. Grey was most concerned about being left out of China after having to defer to Wilson on Mexico.

Lloyd George does not enter the stage until Gardner moves to the Anglo-American response to Germany and the Russian Revolution. As portrayed by Gardner, Lloyd George provides something of a foil to Wilson's ideological preoccupations with self-determination and the desire to help make the Bosheviks go away. Lloyd George would not have been displeased by a return to the March revolution in Russia but was much more flexible than Wilson on negotiating with the Bolsheviks. The most original section of this study comes after Wilson's collapse and his defeat on the League of Nations. When Lloyd George needed Wilson's support for a new policy of dealing with the Bolsheviks based on national interest, Wilson slid into a moral embargo of the Soviet Union. The Republicans continued this stance, and Lloyd George failed to come to terms with Moscow at the Genoa Conference. Wilson tried to resist the continuing pressures (which Lansing applied) to throttle the Mexican Revolution. Ultimately, Wilson passed the issue unresolved on to the Republicans, whose policy on China, like Wilson's, initially worried London. It ended up, however, creating the Washington system, which stepped back a bit from Wilson's liberal hopes for China.

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MALCOLM H. MURFETT. *Fool-Proof Relations: The Search for Anglo-American Naval Cooperation during the Chamberlain Years, 1937–1940*. Kent Ridge: Singapore University Press. 1984. Pp. xix, 324. Cloth $22.00, paper $17.00.

In 1924 Admiral David Beatty, the British naval hero of the First World War, warned that, in the absence of an American fleet in the Western Pacific, Britain could preserve its imperial position in the Far East only on the sufferance of another power: Japan. Unfortunately, in the 1930s Japan abandoned its restraint, and soon Britain's relations with Germany and Italy had also deteriorated. Britain could not meet this triple challenge unaided. Malcolm H. Murfett explores the debate from 1937 to 1940 within the Chamberlain cabinet, the Foreign Office, and Admiralty over the possibility, desirability, and timing of approaching the United States on the question of naval cooperation, especially in the Far East. This study is soundly based on British primary sources, but, despite references to American archives (and the title of the book itself), Murfett does not examine American policy in detail. On the British side, however, Murfett can justly claim that he has explored the naval dimension down to the resignation of Chamberlain in May 1940 in greater depth than other scholars have.

Murfett sensibly concludes that Chamberlain's replacement by Churchill did not mark a watershed in Anglo-American naval relations and that the main determinant of those relations was the degree to which the Axis threatened the United States. Chamberlain himself, even before March 1939, was not invariably an obstacle to talks. Indeed, Murfett argues that, though full of good intentions, Anthony Eden, foreign secretary until the beginning of 1938, was too impulsive and ambitious in his search for some kind of cooperation. Murfett usefully describes the bewilderment of British ministers, diplomats, and admirals as they tried to make sense of American policy and politics. But in the process he has not always made sufficient allowance for the problems of Roosevelt and his team as they tried to lead America away from isolationism and to comprehend what was happening in Europe and the Far East. Both governments made mistakes, but Murfett is too often tempted to see problems through the eyes of the British government. Perhaps the omission from his bibliography of Robert Dallek's admirable study of Roosevelt's foreign policy helps explain his frequently critical view of the president and his tendency to attribute more seriousness to some of Roosevelt's ideas than they strictly deserve. Roosevelt spent much time thinking aloud and experimenting simultaneously with several possibile lines of action. Some found his methods confusing and exasperating; others found them stimulating. Of course, many of Roosevelt's ideas were impractical, but he can, for instance, be credited with perceiving the need for large-scale replenishment at sea as early as 1937. Murfett, for his part, produces an informative table of the operating capabilities of major British warships at various

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distances from their bases, but he fails to develop this interesting point in his text. Indeed, the book on several occasions leaves one desirous of more analysis and information. But, within its limits, it is a useful addition to the literature on this period.

C. J. BARTLETT
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ANCIENT

MUHAMMAD A. DANDAMAEV. *Slavery in Babylonia: From Nabopolassar to Alexander the Great, 626–331 B.C.* Translated by VICTORIA A. POWELL. Edited by MARVIN A. POWELL and DAVID B. WEISBERG. Rev. ed. DeKalb: Northern Illinois University Press. 1984. Pp. xxv, 836.

Since Western Orientalists ordinarily have not learned Russian, the translation of Soviet scholarly classics into English deserves warm gratitude. That is especially so when a scholar of the eminence of Muhammad A. Dandamaev is concerned, for some of his works have already reached the West through German. Victoria A. Powell, the principal translator of this book, is not an Orientalist but enjoyed the guidance of Marvin A. Powell and David B. Weisberg. As a result, we have the advantage of experts in Russian, history, and philology. Gratitude is owed, also, to Northern Illinois University Press, which produced the volume in a handsome way.

Dandamaev presents a synthesis based on his own mastery of more than ten thousand documents. The translation, moreover, encompasses publications beyond its original Russian edition of 1974. We thus have a source book of unusual breadth; more than two hundred cuneiform documents are translated as well. The book deals with a period of considerable change: with the end of the Assyrian empire and the creation of the vast Persian one, an international economy succeeded the provincial and isolated economies of the Near and Middle East. Babylonia, then as later, formed the meeting place of economies, situated as it was at the center of a number of important trade routes. Mediating trade from the Aegean to the Indus, from southern Egypt to Turkestan, that cosmopolitan area and its records testify to the beginnings of a genuinely international market for both goods and ideas.

Dandamaev treats slavery in private households, temple slavery, royal slaves, and dependent social groups. Contrary to former views, he proves that slaves did not move freely from one status to another but were the property of their owners, having only obligations and no rights. Slaves in agriculture were mainly tenant farmers; large numbers worked on temple estates. But slavery did not play a decisive role in agriculture: "The forced labor sector in Babylonia, in contrast to Greek and Roman Antiquity, was not able to absorb such masses of captives." Dandamaev notes that "no modern concept of class can be applied to Babylonian evidence without extensive qualification." He distinguishes three socioeconomic classes: first, persons who own property in the means of production but did not engage in productive labor; second, persons who possessed means of production, were engaged in productive labor, but did not exploit the labor of others; and, third, persons who constituted the sector of compulsory labor.

Dandamaev takes up the broader agenda of Soviet scholarship in the humanities, as is to be expected, but in no way does he regard as a matter of dogma opinion to be tested against data. "In Soviet literature on the Ancient Orient the opinion is widespread that the societies of the Ancient Near East constitute the earliest states of slave society. . . . [But] slavery never reached in Babylonia such a degree of development that one can speak of slave labor as having the leading role in the economy. Slave labor was only one of several types of forced labor and not always the most significant."

This masterful work takes its place alongside only a handful of Soviet classics available in English. Among other Soviet studies of slavery, I call attention to *Soviet Views of Talmudic Judaism: Five Papers by Yu. A. Solodukho* (1973), a work of a different order but of equivalent freshness of perspective.

Dandamaev's work provides a definitive account of a subject critical to the understanding of society, and the translator and editors, as much as the author, deserve thanks.

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SARAH B. POMEROY. *Women in Hellenistic Egypt: From Alexander to Cleopatra*. New York: Schocken. 1984. Pp. xxii, 241. $16.95.

This study combines Sarah B. Pomeroy's two interests: women's studies and papyrology. The sources include historical works and inscriptions as well, but it is fair to say that without the progress of papyrology in the last fifty years this book could not have been written. In fact, it is instructive to compare it with an earlier attempt in the same field, Grace McCurdy's *Hellenistic Queens* (1932). McCurdy confined herself to queens: high-profile women who played a role that would have been unthinkable in Periclean Athens. Pomeroy devotes one chapter only—the first—to queens, who "set the fashion for other upper-class women in Alexandrian society" (p. 40). They followed a tradition derived from their homeland Macedonia, although it is well to remem-

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ber that the Greeks from Herodotus to Diodorus thought that greater equality of the sexes in Egypt was a mark of difference between the Greek and the Egyptian worlds.

The second chapter, on Alexandrian women, is a remarkable compendium of information gleaned from a great variety of sources. Citizenship laws in Alexandria encouraged the rearing of daughters—which was not the norm in the Hellenistic world—and restricting intermarriage with non-Greeks. But, in fact, good evidence for Alexandrian citizenship comes only from the Roman period. The bulk of this chapter deals with famous women of Alexandria: royal mistresses, priestesses, women philosophers (there seems to have been a tradition of women philosophers in Alexandria down to Hypatia, who was killed in the early fifth century A.D.), and, finally, Alexandrian women in literature. Theocritus is the most famous literary witness, but he is only one.

The third chapter, "Some Married Women in the Papyri," is an essay on the papyrological evidence for the legal obligations of husbands and women's rights concerning contracts, marriage, and dowries. Soldiers' wives deserve a special section; Ptolemy I seems to have been the first employer of mercenaries to realize that an effective way of keeping the mercenary loyal was to see that his wife and children did not accompany him on campaign but stayed at home. The last half of this chapter deals with the case of a woman between two cultures, the wife of the soldier Dryton. Unlike her husband, she had two names—one Greek, one Egyptian—and must have been half-Egyptian herself, whatever the actual makeup of her genes. Dryton wrote four wills, three of which survive more or less, and they are remarkable for their hard-headed attitude toward his wife, though she herself was no mean businesswoman.

The last two chapters deal with women in the economy. The first, "Slaves and Workers," is to some extent controversial. Slaves are hard to identify as such in the papyri, and probably their actual number was small in Egypt compared to other parts of the ancient world because free labor was cheap. Still, the Zenon papyri provide some good evidence for the slave trade and also for the last chapter, "Women's Role in the Economy." It is hard, however, to identify work that is exclusively female in Egypt. Zenon could write that he had 784 female wool workers in three villages in 250 B.C., but men worked wool, too. At this level, there was a degree of equality.

Pomeroy's study is written with a fine command of the primary sources and secondary works. It deserves high commendation.

J. A. S. EVANS
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M. SPEIDEL. *Roman Army Studies*. Volume 1. Amsterdam: J. C. Gieben. 1984. Pp. 436.

More often than not volumes of this sort, especially if they reproduce articles previously published, are at best a convenience to scholars in the field. *Roman Army Studies* is an exception. Although the longer papers were published in major journals, most of the more specialized studies appeared in relatively obscure publications that are available in only a few research libraries. If only to ensure wider circulation of M. Speidel's views, republication of the significant articles he produced between 1970 and 1983 would be justified. A case can be made, moreover, that this collection of thirty-seven papers—two of them previously unpublished—defines the main directions of research on the Roman military during the past decade more clearly than recent studies of imperialism and grand strategy.

The essays cover a broad spectrum of issues—recruitment, career patterns, composition of ethnic units, acculturation in the frontier zones—that determined the deployment and effectiveness of the imperial army. Less interested in the evolution of imperial policies than in the internal history of the Roman army, Speidel has selected for analysis a number of epigraphic and papyrological texts, some recently discovered, that bear on the composition and various activities of military units in specific provincial settings. Implicit in his microanalytic approach—which Emil Ritterling and Eric Birley, the masters to whom the volume is dedicated, would find congenial—is the conviction that an understanding of the Roman military is less dependent on the formulation of questions imported from other disciplines than on the proper evaluation, or reevaluation, of the ancient evidence. In the hands of a less talented scholar this predilection might reduce the inquiry to idiosyncratic exercises. In his pursuit of details, however, Speidel rarely loses sight of the broader historical objective. Some of the topics, to be sure, border on the pedantic, but even in the more restricted essays he has attempted to point up the significance of the text under discussion by placing it in a wider historical and historiographical context.

More could have been done, however, to prepare the reader for the task. The decision to reproduce the articles in their original form, augmented only by brief discussion ("addenda") of more recent literature, is understandable; the failure to produce a substantive introduction is not. Without some assistance from the author, even the initiated will find it difficult to discover a common theme or purpose in the apparently disparate essays. One hopes that Speidel will seize the next opportunity—in the second volume of essays he has promised, if not be-

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fore—to offer readers a more synthetic statement of his perception of the Roman army.

JOHN W. EADIE
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JUDITH P. HALLETT. *Fathers and Daughters in Roman Society: Women and the Elite Family*. Princeton: Princeton University Press. 1984. Pp. xix, 422. Cloth $40.00, paper $12.50.

"The hand that rocks the cradle rules the world." This old adage, long predating women's liberation or even the suffragette movement, encapsulates the "paradox" that forms the kernel of Judith P. Hallett's book: "that many well-born [Roman] women are remembered as possessing forceful personalities and exerting a substantial impact on men's public affairs, despite their society's extolling of domesticity as women's only proper concern, and despite their own legal disabilities and formal exclusion from political participation" (p. 6). But, if this is a paradox, it is by no means confined to Roman society, and its appearance in classical Rome is by no means its earliest manifestation. A characteristic anecdote retailed by Plutarch attributes a similar sentiment to the Athenian general and statesman Themistocles, who lived at a time when the Roman Republic was in its infancy. According to this story, Themistocles claimed that his young son was the most powerful person in Greece, because the Athenians were the most powerful of all the Greeks, Themistocles himself was the most powerful Athenian, Themistocles' wife ruled Themistocles, and their son in turn ruled her!

The tone of this sage observation is, of course, jocular, and it belongs to those anecdotes that, whether true or merely *ben trovato*, obviously contain a grain of truth but not necessarily any more than that. The real test is whether women exercised genuine political influence despite their lack of formal power and, if so, in what direction this influence was exerted. Yet somehow, despite her work's ample length, Hallett never quite comes to grips with this central problem.

The book is divided into three sections, dealing respectively with women as daughters, women as sisters, and women as mothers. It thus omits altogether any treatment of what must undoubtedly be the most likely source of feminine influence on male decision making: women as wives. The result is, perhaps inevitably, anecdotal rather than analytical. That aristocratic Roman men adored their daughters, respected their sisters, and loved their mothers is no doubt reassuring but not terrifically illuminating.

One of the most delightful (and least reliable) of the anecdotes related by Hallett (p. 249) if anything militates against any sign of genuine female political influence. A young boy sworn to secrecy after being allowed to attend a Senate meeting was nagged by his mother to reveal what he had heard. Instead of telling the truth, he had a bit of fun at her expense by claiming that the debate had been on the question of whether it was preferable for a man to have two wives or for a woman to have two husbands. The next morning the Senate was besieged by hordes of female demonstrators clamoring in favor of polyandry!

M. T. W. ARNHEIM
University of the Witwatersrand

MIRIAM T. GRIFFIN. *Nero: The End of a Dynasty*. New Haven: Yale University Press. 1984. Pp. 320.

Since the genre of imperial biography has fallen into disrepute among Roman historians in recent years, Miriam T. Griffin appears to have taken certain risks in bringing out a life of Nero at the present time. In actuality, however, the risks are minimized by two factors: first, the success of her earlier, related study of Seneca and, second, the tactic she employs of not simply constructing a chronography of Nero's life and reign but instead evaluating Nero's personality against the pressures imposed on him by the principate as an institution and thereby providing a historical explanation of the demise of the Julio-Claudian dynasty.

In part 1 of the book Griffin builds up her portrait of Nero's personality and describes the political character of his regime. Not surprisingly, she finds Nero to have been weak, insecure, vindictive, anxious for acclaim, and even paranoid, more committed to literature and the arts than to consistent cultivation of the Senate and the legions; in addition, she considers that his rule degenerated from an auspicious "Augustan" beginning into a reign of terror. Much of this inevitably follows the general consensus of the ancient literary sources, especially Tacitus, but Griffin strengthens her presentation by paying particular attention to Neronian coinage and the archaeology of the Golden House, so that overall there can be no question about the main lines of development she offers.

Griffin is more innovative in part 2 of her book, where she argues that Nero's temperament clashed with inherent features of the principate to bring about the emperor's fall from power and the end of Rome's first imperial dynasty. On the issue of succession, the problematically high number of potential rivals to Nero, whom he brutally, almost systematically eliminated, was owed to the long survival of the Julio-Claudians, and, although Nero recognized the need to display liberality, megalomania pushed his spending beyond the limits of available re-

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sources. His natural philhellenism, unpalatable in the West, encouraged him to seek applause in the Greek East, where, however, the traditions of absolutist rule were deeply entrenched. Most important, however, Nero failed to satisfy the demand for victories expected of him, except in the inappropriate realm of aesthetic competition. Only thirty-one at his death in A.D. 68, Nero was thus never able to justify himself as Rome's emperor and never came to terms with the obligations of his position.

Although scholars will need to supplement Griffin's chapter on Neronian literature with John P. Sullivan's new book (*Literature and Politics in the Age of Nero* [1985]) and some account of another recent biography by Eugen Cizek (*Néron* [1982]) should be taken, Griffin's book will become the standard reference on Nero, not least because of its balance and sanity of judgment. Yet a flaw may still persist, namely, the difficulty of producing a truly comprehensive and integrated account of Nero's personality given the absence of strictly contemporary literary sources and, especially, of any private record from Nero himself. Since there is nothing comparable to the correspondence of Cicero for Nero, it is impossible for the modern historian to escape the bias of the Neronian tradition inherited and compounded by Tacitus and Suetonius. Thus, if Griffin's book represents an undoubted advance on other biographies, nevertheless much of Nero's personality must of necessity remain not just enigmatic but, in the end, unknowable.

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S. R. F. PRICE. *Rituals and Power: The Roman Imperial Cult in Asia Minor*. New York: Cambridge University Press. 1984. Pp. xxvi, 289. $49.50.

S. R. F. Price offers a fresh appraisal of the cult of the Roman emperor from a perspective that views "both religion and politics as parts of a web of power" (p. xi). Price's study concentrates on Asia Minor, and his catalogue of 156 imperial temples and shrines as well as his maps of their location are of considerable reference value. The first part of the study deals with the background of Hellenistic ruler cults, the relations between Greeks and Romans, the distribution of the evidence for the cult, and the role of imperial festivals in the cities. The second part, "The Evocation of Imperial Rituals," surveys the archaeological and inscriptional data for the architecture, images, and sacrifices of the cult. A concluding chapter interprets the significance of the cult of the Roman emperor in the context of "rituals, politics, and power."

Price warns the reader against cultural biases such as "Christianizing" assumptions (p. 2) and takes issue with many judgments about the imperial cult that have gained currency among scholars. He denies that the cult was just a matter of political expediency or a manifestation of religious decadence. It was a significant institution created by the Greeks of Asia Minor to relate themselves to the new Roman power. He rejects the notions that the cult was only of interest to the ruling elites and that the latter were religious skeptics. In examining the Hellenistic background, Price finds that, contrary to expectation, the Roman imperial cult finds its closest affinities to the divine rather than to the hero cults. Once established, the imperial cult spread rapidly throughout Asia Minor.

Price's exposition also sheds light on New Testament passages and on early church history. He maintains, for example, that the Jews and Christians differed with respect to sacrifices for the emperor (although both objected to sacrifices to the emperor) because of different attitudes to the phenomenon of sacrifice itself. At least until the beginning of their revolt against the Romans in C.E. 66, the Jews could accommodate sacrifices for the emperor within their other sacrifices; Christians believed that the death of Christ was the final sacrifice.

Price concludes that in many respects the emperor was treated like a god. For example, imperial and divine temples, shrines, and images were called by the same names and also looked alike. But there were some differences or hesitations. Sacrifices were most commonly made on behalf of the emperor but rarely to the emperor. We have no evidence as yet that any petitionary sacrifices (like requests for healing) were offered to the emperor: "Language sometimes assimilated the emperor to a god, but ritual held back" (p. 213).

The greatest contribution of this work is the thorough investigation of the relevant archaeological and inscriptional data viewed from a new standpoint. Some interesting but occasionally questionable attempts to transcend the barriers of Roman Asia Minor with references to Egypt, the Byzantine church, Victorian England, Cambodia, Africa, and social psychology display evidence of wide reading but are distractions rather than indications of an integrated interdisciplinary approach.

Finally, in objecting to the dichotomy that drives a wedge between "personal religion" and the "political" imperial cult, the characterization of both politics and religion as "ways of systematically constructing power" (p. 247) is a truism that locates the imperial cult on a continuous spectrum but does little to acknowledge the essential differences between religion and politics.

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JOHN MORRIS. *Londinium: London in the Roman Empire.* Revised by SARAH MACREADY. London: Weidenfeld and Nicolson; distributed by St. Martin's, New York. 1982. Pp. xvi, 384. $27.50.

C. J. ARNOLD. *Roman Britain to Saxon England.* Bloomington: Indiana University Press. 1984. Pp. ix, 179. $22.50.

In the recent spate of books on the Roman city of London, the most individual is the volume on which John Morris was working in the last years of his life. Morris was an engaging and inspiring teacher, endowed with intellectual qualities that do not easily transmit themselves to the printed page. His book on *Londinium* reveals his wide reading and learning, his readable prose, and his lively interest in the human beings behind the archaeological and historical fragments. It also reveals, however, an often willful use of sources and a tendency to let his learning run away with him, sometimes down strange paths. The historical sources are often made to say much that is impossible to accept, and his use of the secondary works is frequently careless. Still, much in these pages is interesting, even compelling; for Morris was trying to show Londinium as one of the great cities of the western provinces, as part of the great organism of empire, not merely as the largest of Romano-British cities. His most successful sections are those that center on this theme.

Pre-Roman London can hardly be said to have existed, so Morris is compelled to give a brief summary of the character of later prehistoric Britain. Heavily influenced by the notions, in vogue twenty years ago, of Belgic invasion, this summary and a later chapter on "Belgic" Britain have a decidely dated air. The conquest of Britain, in which London played a role that is still shadowy, is treated in a rather loosely argued chapter that adds nothing to what has been set forth by others. After accounting for the foundation and early years of London, Morris returns to affairs of empire, specifically to Flavian Rome, and then turns back to Flavian Britain and the northern conquests of the Flavian governors. Thus, one does not reach the core of the book, the city of Londinium, until after perusing one hundred fifty pages, few of which are really necessary. The life of the city is presented under a number of thematic headings: government, learning and religion, sport and leisure, food and work. Finally, we have the close of Roman London in the fifth century, long a controversial subject and one on which important information has come to light since Morris's death. The treatment is, in general, expansive and the style often prolix. The book provides entertainment in plenty for those who enjoy discursive history, but the student seeking information that can be solidly supported will find the narrative confusing and often misleading.

Sarah Macready, who has seen this book to press, has performed an unenviable task with great skill. The temptation to rewrite and edit severely must at times have been overpowering. That the book has shape and character is largely owing to her efforts, and she must regard her duties as honorably discharged. Authors and publishers should now give Londinium a long rest and wait for the emergence of the huge backlog of unpublished material on sites. Only then will it be clear how much we know about Roman London.

If Morris could manipulate historical sources to accord with his arguments, C. J. Arnold goes to the other extreme and largely ignores them. The reader will look in vain for Bede, Gildas, and the Anglo-Saxon Chronicle in the (admittedly inadequate) index of this odd essay. Only in the last few pages does the author face up to a central problem in the study of this period of British history: the integration of historical and archaeological evidence. But then he thinks better of it and ends his book. It is difficult to divine what Arnold intended to achieve. For the most part we are confronted with a hopeless jumble of facts, assertions, speculations (some of them second-hand), and unvarnished errors. Aside from his failure to deal with the literary sources, Arnold has fastened on to several very dubious recent discussions of archaeological evidence (for instance, on the end of Romano-British cities and towns) and elevated their conclusions to the status of evidence. This is bad enough, but it is all made worse by poor writing. Arnold makes little attempt to argue a case: the dubious proposition soon becomes a recognized fact. A further fundamental weakness of the book is that no real attempt is made to distinguish between different regions of Britain, such as those that saw early Germanic settlement, those that experienced it first after A.D. 500, and those that were never widely settled by the Anglo-Saxons. It is high time that writers on this period admitted that half of what had been Roman Britain did not pass under English domination until some five centuries after the end of Roman rule.

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E. A. THOMPSON. *Saint Germanus of Auxerre and the End of Roman Britain.* (Studies in Celtic History, number 6.) Dover, N.H.: Boydell and Brewer. 1984. Pp. x, 127. $29.50.

Fifth-century Britain has always held a great fascination for historians and archaeologists. At one time it was considered so impenetrable that it was a serious hiatus in the historical sequence. This was

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partly owed to the scant literary evidence, which was difficult to interpret, as well as to the problem of dating for the archaeologist without either coins or pottery. E. A. Thompson has over the years been a pioneer in demonstrating the real potential of the surviving records when they are subjected to critical examination. This book is an excellent example of the Studies in Celtic History series. It is neither a biography of Saint Germanus of Auxerre nor an account of the historical background but, after a short introductory chapter, a serious study of the biographer Constantius and the value of his evidence pertaining to Gaul and Britain in the early fifth century, which was written a half century later.

Thompson's general conclusion is that Constantius may have known a great deal about Gaul, but of Britain he was woefully ignorant. This presents a serious problem in his account of the visits Germanus made to Britain in A.D. 429 and 437 to combat the Pelagian heresy. Although the Britons had withdrawn from the Roman Empire about A.D. 410, the church still considered British Christians to be under its care. The Christians in Britain were mainly town dwellers, and Celtic paganism continued to be practiced in the countryside. In fact, the name "pagan" was derived from *pagani*, the country people.

Pelagius was a Briton who went to Rome about A.D. 380 to study law but became a priest and developed the concept of free will. Saint Augustine of Hippo, a much greater theologian, saw this as an impudent substitution of human reasoning for divine omniscience, and he successfully used all his powerful influence to stamp out this pernicious heresy.

Thompson is not concerned with the theological debate but only with its effect in bringing Saint Germanus to Britain to lure the wayward heretics back into the fold of the true church. Because the details of this visit are obscure, much thought is given to them in reflecting the conditions in the country at that time, but it seems a little like squeezing a dry cloth.

In chapter 7 Thompson turns to the second visit and convincingly links the date with an Armorican revolt. Chapter 10 is a brave attempt to bring some sense and order to the sequence of events in the second half of the fifth century. Here Thompson is not so successful, for, although he firmly grasps the difficulty of the literary evidence, he appears to have less understanding of the growing body of archaeological evidence, especially of the Saxon settlements in the eastern parts of Britain. These settlements appear to have been part of a deliberate policy of Rome that was continued by British rulers; Thompson, however, specifically rejects this view (p. 97).

This is a valuable and stimulating study for all those who are closely associated with the period, but the general reader may find it daunting.

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MEDIEVAL

JÜRGEN ZIESE. *Wibert von Ravenna: Der Gegenpapst Clemens III., 1084–1100.* (Päpste und Papsttum, number 20.) Stuttgart: Anton Hiersemann. 1982. Pp. 307. DM 160.

With this volume, the series Päpste und Papsttum has published its first biography of an "antipope." Jürgen Ziese describes Wibert of Ravenna as the "most successful imperial antipope of the Middle Ages" (p. 275), and one cannot quarrel with his carefully worded encomium. There were other important antipopes—for example, Anaclet II (1130–38) and Clement VII (1378–94)—but they were not creatures of the emperor. (And some historians, if not the list of the *Annuario Pontificio*, would not consider Clement VII an antipope!) At the time of Wibert's election, the papacy had only recently eliminated imperial participation in the choosing of a pope through the electoral decree of Pope Nicholas II (1059). During Wibert's pontificate, however, a version of this decree circulated that returned to the emperor a role in papal elections. In the late 1080s and 1090s it must have seemed that the issue was still not definitively resolved, and only Frederick Barbarossa's futile efforts to create his own line of popes (1159–80) ended any hope of imperial influence or rights in the process of selecting Peter's successors.

Ziese's biography covers Wibert's entire career: his background, a short career in the imperial chancellery, election as archbishop of Ravenna, and, finally, his election as pope in 1084. Ziese argues that Wibert's success was owed to his juridical and administrative training, his experience in the *Reichskirchensystem*, and, in particular, his military abilities.

The sources for evaluating Wibert are sparse, and Ziese has not, to judge from this book, explored the archives that might have illuminated a dark corner here or there. He argues that Wibert was not simply a pawn of Henry IV but pursued policies that sometimes conflicted with imperial wishes. He emphasizes Wibert's sense of collegiality, sensitivity to episcopal rights, and interest in reform. Wibert did not oppose all elements of Gregory VII's program.

The book challenges received opinion in several ways. Ziese places Wibert's break with Gregory later than earlier historians have. He thinks that Wibert was not deposed from his office until 1078 and, even

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then, was not the most important leader of the Gregorian opposition. Perhaps Ziese's most important—and most convincing—contribution is his analysis of the Synod at Brescia in 1080. He shows that Wibert was not elected pope during this synod but only designated as a candidate for the papal throne.

Although Ziese is often short with the opinions of others, he himself constructs imposing edifices with material from which more cautious historians would have fashioned dwellings less grand. He has a pervasive tendency to overinterpret the evidence, and I suspect not all readers will be convinced by all his conclusions. Nevertheless, it is good to have a new biography of Wibert (the last appeared in 1888), and ecclesiastical historians will profit from this "brief" for Wibert. With this book and Ingrid Heidrich's recently published biography (*Ravenna unter Erzbischof Wibert (1073–1100): Untersuchungen zur Stellung des Erzbischofs und Gegenpapstes Clemens III. in seiner Metropole* [1984]), Wibert is now particularly well served by current historiography.

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STEVEN EPSTEIN. *Wills and Wealth in Medieval Genoa, 1150–1250.* (Harvard Historical Studies, number 103.) Cambridge: Harvard University Press. 1984. Pp. viii, 269. $22.50.

Steven Epstein's detailed and often ingenious examination of all extant Genoese wills of the years 1155–1253 demonstrates once more the importance of testaments for the study of family relationships, inheritance patterns, and charity preferences. The 632 wills—many of which are inedited—reflect widely differing levels of individual wealth and may therefore be considered as representing a substantial segment of Genoa's population. Men made 323 of the wills; women, 309. Consequently, the wills stand out among the sources of social history for their lack of the usual numerical bias in favor of men.

The wills disclose that the principal heirs were predominantly members of the nuclear family. The analysis of the four wills for which corresponding inventories exist suggests that the principal heirs received the bulk of the estate. The average number of children per couple was 1.87. Of the testators who had sons and daughters, 57 percent favored the sons, 6 percent favored the daughters, and 37 percent divided the inheritance evenly among their children. Husbands often attempted to ensure that their wives remained widows, but women did not impose similar restrictions on their husbands. The mean burial and funeral expense was L. Gen. 3; the mean charitable bequest was about L. Gen. 5. Donations for charity became more extensive in the latter part of the period under consideration, with socioeconomic groups differing in their preferences for the various charities. Surprisingly, only 7 of the 632 testators mention usury. These are some of the highlights of Epstein's clear and well-argued exposition, which is often spiced with humor.

The many data Epstein derives from the wills significantly contribute to the understanding of Genoese society and economy in the twelfth and thirteenth centuries. But the absence of the raw material prevents the reader from pursuing his own queries, such as whether the number of children per couple, partiality to sons over daughters, and the usage of female names underwent changes between 1150 and 1250 or varied according to the testators' wealth. This deficiency, common to most recent studies of this type, could be largely remedied by appending to the volume summaries of the wills, perhaps in tabulated form.

Epstein rightly insists on the uniqueness of Genoa's wills: indeed, in no other European city have so many wills survived from so early a date. But a glance across the Mediterranean reveals another substantial body of wills, some contemporary with those studied by Epstein and some older by a century or more. These are, of course, the wills of the Cairo Geniza, studied by the late Shelomo Dov Goitein in *A Mediterranean Society* and especially in "Disposition in Contemplation of Death—A Geniza Study" (*Proceedings of the American Academy for Jewish Research* 46–47 [1979–80]: 155–78). Even a cursory comparison discloses remarkable similarities between the Genoese and Geniza wills and underlines once again the desirability of including the Geniza within the purview of students of the social history of the Italian maritime cities of the time.

BENJAMIN Z. KEDAR
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JOACHIM GÖBBELS. *Das Militärwesen im Königreich Sizilien zur Zeit Karls I. von Anjou, 1265–1285.* (Monographien zur Geschichte des Mittelalters, number 29.) Stuttgart: Anton Hiersemann. 1984. Pp. vi, 335.

Joachim Göbbels is a student of Peter Herde, who is well known through his biography *Karl I. von Anjou* (1979). Göbbels's work reconstructs the Anjou military system, which was essential for winning the former Norman and Hohenstaufen kingdom composed of the south Italian mainland and the island of Sicily and for holding on to the mainland rump after the loss of Sicily to the House of Aragon through the Vespers of 1282.

Using Riccardo Filangieri's reconstruction of the lost records from the state chancery of Naples (*I registri della cancelleria angioina* [1950–81]), other

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sources, and the rich secondary literature, Göbbels gives a detailed account of the three services—the army, the castles, and the navy—in the context of the crises of the time. Charles aimed at a strong centralization of these services through three crown officers—the *comestabulus* for the army, the *magister balistariorum* for the castles, and the *admiratus regni Siciliae* for the navy—to improve the control over his alien kingdom and to prepare for his offensive against Byzantium. Military and political setbacks, however, forced Charles to delegate authority further down the echelons. The failures in Albania, the Vespers, and the Aragonese landings on the mainland after the Sicilian revolt, particularly of the Almogarvs (the Aragonese irregulars), had to be dealt with by other officers with specific offensive or defensive missions—the captains-general, the *provisores castrorum*, and the vice admirals. Göbbels carefully traces their changing position.

As strong centralization failed, multiple problems encumbered the military system. Charles had to guard against dishonesty and disloyalty by appointing officers to check on each other. His relations with the Italians in his kingdom worsened after many defected to the side of the Hohenstaufen claimant, Conradin, in 1268 and after the Vespers. The more he excluded them from high military posts, the less he could trust them. Yet as mercenary soldiers the Italians were an important part of his army, and, as port commanders (*prothontini*) and naval ordinance officers (*comiti*), they were indispensable because of their know-how. The Saracens of Lucera were a special case, to which Göbbels pays particular attention. They were important because of a tax they paid and because of the longbow men they furnished, but they could not be trusted because of their Hohenstaufen loyalties. Fiscally, the military establishment of Charles I was hybrid, with paid service overshadowing feudal service. Hence, it furnishes important parallels with the development of the money-fief and indenture north of the Alps.

Blending the reconstruction of the military service with contemporary politics and war, and doing this at all echelons, creates a problem of repetition that Göbbels does not always resolve happily. Brief references would have been preferable to restatements of facts. For example, the author should not have included the quotations, dissonant at that, from the commission of Vice Admiral Jacques de Burson on pages 185 and 225. The text is correct on page 225, while the footnote is correct on page 185. The book required more careful scrutiny on the part of the publisher's reader. Nonetheless, *Das Militärwesen im Königreich Sizilien* is essential reading for the military historian and for the student of the Italian south.

REINHOLD SCHUMANN
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ALAN M. STAHL. *The Merovingian Coinage of the Region of Metz*. (Publications d'Histoire de l'Art et d'Archéologie de l'Université Catholique de Louvain, number 30; Numismatica Lovaniensia, number 5.) Louvain: Institut Supérieur d'Archéologie et d'Histoire de l'Art Séminaire de Numismatique Marcel Hoc. 1982. Pp. vi, 200.

This study undertakes to examine one angle of the Merovingian economy through the coins of one area, in order to derive from them a better idea of their source, scope, and purpose. More than simply reviewing known material Alan M. Stahl builds his work on three new investigations. He defines the monetary area in question and collects a great many new coins, calculates a gold fineness of many pieces through specific gravity, and proposes a computer-guided method of arranging the material into chronologically sequential "Clusters." The Catalogue, which gathers the civic coinages for Mettis and the surrounding region, is a considerable advance over the work of Maurice Prou, whose listing Stahl has more than doubled, though the relatively few instances of die-linkage suggest that much remains to be discovered. The coins are arranged in ten Groups according to apparent type-derivation and development. The steady decline in mean gold fineness of the Groups seems to support their chronological seriation, when they are compared to the chronologically better understood issues of the Merovingian kings, whose fineness also declines over time. A second arrangement of his material in Clusters, accomplished by a computerized comparison of all the coins with one another, produces a second, differing chronology, which also can be set against the regal. In the chapter "Coinage and Urban Settlement" Stahl sedulously collects all manner of contemporary reference to Metz, Trier, Toul, and Verdun, showing that the relative importance of Metz in matters political, religious, and commercial corresponds to its predominance in coinage. In "Coinage and Rural Settlement" he considers the relation of minting to local settlement, and concludes that it had less to do with specific commercial activity or intensity of habitation than with regional governance. The concluding chapter argues that the Merovingian coinage and monetary circulation of A.D. 560–660 must have been almost exclusively in gold; that the numerous minters who signed the coins were functionaries only; and that, in spite of the absence of the royal names, coinage was under royal control, the metal probably deriving from the royal revenues.

All of this sounds promising, but the novel undertakings fail in execution. The specific gravities are so loosely calculated that they cannot be used, as the author tries to use them, to construct a chronology of falling gold fineness. There are anyhow two

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different chronologies for the same material, with no indication which is the better or the more likely. The computer-based taxonomy is neither well conceived nor persuasive in its results. Worst, the book teems with errors of detail that undercut the arguments: quantities that do not tally between text and Catalogue, nor even from page to page, histograms whose sums are not those of the quantities in text they are supposed to represent, and self-contradictory chronologies. The errors of omission and commission, many of which could easily have been caught by a careful editor, are much too plentiful to list here. As an example, the results of the highly complicated Numismatic Taxonomy are summarized in one half-page figure: to bring it into consonance with the actual Catalogue listings on which it is supposed to be based requires no fewer than ten emendations in the one or the other.

It is good to have to hand the new material collected for the Catalogue and plates, but the book is untrustworthy. It is also another example of the editorial slackness of this series.

T. V. BUTTREY
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D. L. D'AVRAY. *The Preaching of the Friars: Sermons Diffused from Paris before 1300*. New York: Clarendon Press of Oxford University Press. 1985. Pp. viii, 315. $39.00.

This book is a major contribution to the growing scholarship on medieval preaching. Since the publication of J. B. Schneyer's *Repertorium der lateinischen Sermones des Mittelalters* (1969–75), historians—D. L. d'Avray prominent among them—have tried a variety of approaches to delimit the vast territory of sermon evidence. Studies focusing on single manuscript collections, on individual or small groups of preachers, on sermons to particular groups or audiences, and on particular texts or themes have revealed some of the detailed contours of this rich landscape. Here, in studying the preaching of the friars, d'Avray broadens the focus enough to provide, if not a full map, at least a valuable legend of the territory. Moreover, by concentrating on sermon collections produced at or diffused from Paris, he shows just how important this great center of learning—and of popular preaching (for, as d'Avray argues, these activities, though closely related, remained distinctive)—became in the dissemination of ideas to a lay public throughout Europe.

D'Avray has more to say about the techniques of this diffusion than about the ideas themselves. His primary purpose is to show how the availability and use of near-identical copies of sermon collections enabled preaching to function as a medium of mass communication. And he certainly provides sufficient evidence of how frequently the friars took *vademecum* books of model sermons on their missions to bridge the often-assumed gap between academic and itinerant preaching. These collections, he argues, are not to be read simply as the medium used to instruct the laity. The common factor in the experiences of both clergy and laity, the sermons also bring us as close as possible to popular beliefs and attitudes, to the shared religious culture or mentality of the age.

D'Avray's first two chapters on the background and nature of the medium examine how preaching was transformed by the rapid proliferation of sermon collections and by the rise of the mendicant orders, whose members were trained to use these collections. (The *pecia* system, shown to be so vital to the transmission of standardized texts, is also discussed in detail in an appendix.) Chapter 3 scrutinizes the manifold connections between Paris's dual function as a center of scholastic theology and of popular preaching. The final chapter on the interpretation of sermons is likely to prove the most provocative. Disagreeing with Lester Little, d'Avray first argues that the friars' preaching was not preoccupied with the urban environment, nor was commercial imagery any more prominent than images reflecting the concerns and values of feudal or aristocratic society. When such imagery is found, he insists, it should be interpreted with an eye as much to the language of the New Testament as to the morality of the expanding towns. In its passion for similitudes, d'Avray suggestively observes, medieval preaching is closer to the conventions of modern poetry than prose. And he is at his very best when demonstrating the techniques of interpreting how *topoi*, *distinctiones*, *exempla*, and other key modes of thought or habits of mind function in the friars' sermons.

D'Avray tends to be unduly modest, repeatedly insisting on the provisionality of his conclusions. He is right, though, to emphasize the need for further studies and critical editions of individual sermon collections. Those who undertake them and venture into the territory of medieval sermons will be most grateful for d'Avray's meticulously learned guidance.

STEPHEN C. FERRUOLO
Stanford University

ALAIN BOUREAU. *La* Légende Dorée*: Le système narratif de Jacques de Voragine (d. 1298)*. Foreword by JACQUES LE GOFF. (Cerf-Histoire.) Paris: CERF, for the Centre National de la Recherche Scientifique. 1984. Pp. vii, 282. 92 fr.

The *Legenda Aurea* of Jacques de Voragine was one of the most popular and perduring of medieval

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hagiographical collections, whose vogue persisted virtually down to modern times. In this fascinating book Alain Boureau attempts to disengage the narrative systems at work in the *Legenda* in order to understand its goals, methods, and, ultimately, lasting success.

Written by the Ligurian Dominican at the end of the thirteenth century, the *Legenda Aurea* drew on earlier collections of saints' lives but recast them in a new form, in the process drawing together currents of popular culture, always present to some extent in the creation of saintly cults, and the new, doctrinally oriented clerical culture of the thirteenth-century church. The *Legenda* was designed to serve as a reference manual for preachers, not least among them the Dominicans. And it is the Dominican origin and intended audience of the *Legenda* that help explain the narrative modes selected by Voragine for his presentation of the widely heterogeneous material incorporated into the legendary.

In his analysis of Voragine's narrative systems Boureau borrows the critical vocabulary and interpretive method of Claude Bremond's *Logique du récit* (1973) to demonstrate, as Jacques Le Goff points out in his foreword, not only "the logic of the text, but through it, the logic of an apostolic program and ideological system" (p. i). In the end Voragine's didactic goals and dogmatic concerns—his evangelical and ideological aims—triumphed over the specifically historical narrative operative in the hagiographical vitae that served as sources for the compilation. Although using earlier vitae extensively, Voragine transformed them into exemplary and symbolic instances of a universal, dogmatically defined concept of sainthood, stunting both the individuality of the lives of the selected saints and the narrative presentation of those lives in their particularity and historicity. Whereas the narrative system of earlier hagiographies differed little from other forms of historical narrative, the historical narrative of the *Legenda Aurea* is what Boureau terms a "narration exégétique," one that privileges the dogmatic and didactic elements to be gleaned from the universal history of the life of the church. In this way properly historical narration yields to what Boureau interestingly refers to as "the strategy of the list," an accumulation and "agglutination" of innumerable examples, anecdotes, proofs, and cases chosen for their demonstrative efficacy rather than aesthetic coherence. The result is a new "rhetoric of the list," a rhetoric of ordered accumulation that breaks all the rules of ancient composition but permits Voragine to integrate the traditional hagiography of popular and learned traditions into a new synthesis, which, in being collected and fixed, was thereby brought under clerical control. Boureau argues that the extraordinary popularity of the *Legenda Aurea* may be explained by Voragine's ability to combine these disparate elements into a satisfying synthesis, which permitted the Dominican to preserve much of the traditional narrative of Christian hagiography while subordinating it to the church's apologetic program.

Boureau's treatment of the variety of narrative and nonnarrative elements in the *Legenda Aurea* is much richer and more complex than can be indicated here. He has opened up new approaches to questions of medieval hagiography and narrativity alike, and his book can be read with profit by those interested in either phenomenon. Although densely written and elaborately explicated, it will repay the sometimes heavy claims it makes on the reader's attention and memory.

GABRIELLE M. SPIEGEL
University of Maryland

A. VAN NIEUWENHUYSEN. *Les finances du Duc de Bourgogne Philippe le Hardi, 1384–1404: Economie et politique*. (Université Libre de Bruxelles, Faculté de Philosophie et Lettres, number 90.) Brussels: Université de Bruxelles. 1984. Pp. 556. 1,590 F.

Philip the Bold, duke of Burgundy, founded a state that would absorb the entire Netherlands during the fifteenth century. He was also a dominant figure at the French court, where he directed royal policy for two extended periods. After 1384 he ruled Flanders and Artois in the Low Countries and the counties of Rethel, Nevers, and Burgundy as well as ducal Burgundy.

A. Van Nieuwenhuysen has produced a long and detailed study of Philip's finances over the last twenty years of his life. It is a most impressive piece of scholarship, copiously documented with manuscript sources from numerous archives, especially Lille and Dijon. In the tradition of French historiography that I call the "régime financier" school, the book analyzes every sort of revenue available to the duke and how each was administered. It is better than most works of this type, both because of the heavy use of provincial archives and because the author uses the financial accounts to present a global picture of the duke's true wealth and the economic condition of his possessions.

The first half of the book concerns "ordinary" revenues like manorial and feudal dues, justice, coinage, taxes on commerce, and the duke's share of French royal aids. The second part first deals with extraordinary revenues such as loans, royal gifts, and special subsidies that Philip levied on his subjects and then discusses some of his major expenditures. Philip derived about 20 percent of his revenues from these ducal aids and another 20 percent from aids that were, in principle, French royal taxes. Gifts from Charles VI seem to have accounted for

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about 25 percent of Philip's revenue during his last ten years. He did not borrow a great deal, and, when he did, he seems to have been scrupulous about repayment.

Van Nieuwenhuysen does not stress the importance of royal largesse in Philip's budget, but funds from this source were crucial to many of his political and diplomatic initiatives. Sums promised by Charles VI fell behind schedule and gave Philip a constant excuse to feel aggrieved and to intervene in royal finances.

After a severe plague in 1398, ducal finances declined. Philip became more dependent on royal generosity just as his ascendancy at the French court began to be challenged, and he had to entrust his interests there to some henchmen of dubious character. The author mentions these developments almost in passing, and herein lies the one weakness of her otherwise outstanding book. Philip was an exceptionally important person, partly because of his wealthy and strategic lands but especially because of his status as an influential French prince. He dominated the French government between 1382 and 1388, was excluded from power for four years, and then resumed his authority, only to lose it gradually to his nephew, Louis of Orleans, after 1398. How did these political changes relate to Philip's finances and to the expensive projects that he undertook? His successor would quickly learn the crucial connection between his own financial position and his ability to control the royal government. Van Niewenhuysen has provided us with a wealth of valuable financial data, but the value of this information would have been enhanced considerably by including a few pages of political analysis to relate Philip's finances to the broader context in which he was such an outstanding figure.

JOHN BELL HENNEMAN
Princeton University Library

THOMAS N. BISSON, editor. *Fiscal Accounts of Catalonia under the Early Count-Kings, 1151–1213*. Volume 1, *Introduction*; volume 2, *Accounts, Related Records, and Indices*. Berkeley and Los Angeles: University of California Press. 1984. Pp. xxv, 323; x, 454. $95.00 the set.

The Latin documents that contain the medieval administrative procedures for Catalonia have until recently remained virtually untouched, largely because the abundant extant archival material demands not only paleographical competence but also the rare qualities of care and perception that characterize the present study.

Thomas N. Bisson is to be congratulated on his two-volume work, primarily intended as an edition of the fiscal accounts of the early count-kings (1151–1213). The detailed introduction also provides the general reader with a knowledge of medieval Catalan administrative procedures, and Bisson points out that the little-known material he presents is organized to orientate the reader not familiar with Catalonia.

The four chapters comprising the introduction describe the documents dating from the reign of Count Berenguer IV (1131–62) through that of Pere I, who died in 1213. The accompanying tables classify the accounts and related records chronologically (vol. 1, pp. 7–8), the maps place them geographically (for example, vol. 1, pp. 26–27, 29), and related studies and lists supply supplementary information.

Reference to some of the conclusions that Bisson draws will serve to emphasize the importance of his study. Under Count Berenguer IV fiscal initiatives assumed a predominantly personal character. A knight, Bertran de Castellet, was given the task of preparing the Catalan "Domesday," a survey (covering the area charted in the diagram map on page 29) beginning in Vilamajor in February 1151 and ending in Terrassa in July 1151 or 1152. This survey indicates the seigneurial rights and the diversified economy of northeastern Catalonia, suggesting that the administrative system of a greater Catalonia was nurtured in the rural domains of Old Catalonia. The fiscal personnel, vicars, bailiffs, and their subordinants (*saigs*) were invariably prominent men.

Reorganization of fiscal accounting took place under Alfons I (1162–96): a new tendency toward consolidation was apparent, new initiatives were taken, and the structure was routinized. Royal archives were first referred to as such (vol. 1, p. 97), terms of bailiffs were changed from continuous service to a period of one to three years, and a procedure was developed for the office. By 1190 signs of a professional class of accountants (the *mestre racional* of later years) are evident, and Alfons, in making his own will, reflects the organizing abilities of his fiscal officers by itemizing his bequests. His successor, Pere I, a lavish spender and a constant borrower, largely financed his endeavors through credit, thus introducing a new administrative structure to Catalonia.

Bisson's careful scholarship has ensured that his study of fiscal structure in early medieval Catalonia will find a place on the shelves of every serious student of medieval administrative history.

JILL R. WEBSTER
St. Michael's College
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ROBERT I. BURNS. *Muslims, Christians, and Jews in the Crusader Kingdom of Valencia: Societies in Symbiosis.*

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(Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1984. Pp. xx, 363. $59.50.

This book of essays, mainly about cultural contact among Muslims, Christians, and Jews, is the latest volume of Robert I. Burns's ongoing exploration of the society and culture of the thirteenth-century Kingdom of Valencia. Those portions dealing with Christian-Muslim relations add to the picture of intense pressure for acculturation placed on the Muslim minority, even in the absence of overt conflict in much of the period. Burns makes clear that a nonconflictive ambience did not preclude intolerance. The resultant picture of cultural exchange among members of two exclusivist high cultures highlights the social components of acculturation. Muslims adapted to Christian domination not because they sought to assimilate elements of the dominant culture but because their institutions suffered inevitable permutations by virtue of being embedded in an alien administrative system whose directors misperceived most elements of the subordinate group's social structure.

In the informative introductory essay on the historiography of the Muslim minorities in medieval Spain, Burns makes clear his differences with those scholars, mainly Islamists, who argue that Islamic culture remained basically intact under Christian domination. Burns, on the contrary, stresses discontinuity: within one generation, he argues, the nature of Islamic society was severely altered. A chapter on bilingualism defeats the claim of another group of historians, of a nationalist bent, who have asserted that the Muslims of Valencia were bilingual and conversant with the romance language; the thirteenth-century evidence leads Burns to conclude that the vast majority of subject Muslims were monolingual Arabic speakers. Language and religion were doubtless the primary cultural markers in medieval society, but, even while preserving these, the *mudejars* saw their traditional institutions eroded with surprising rapidity.

There is an ingenious twist to this book: many of the chapters pull together pieces of information dispersed through so many documents that only someone like Burns, who has mastered an entire documental universe over the past two decades, could fashion them into a coherent representation. Like an archaeologist, he pieces together these documental sherds to create wholly unexpected artifacts. Two examples will suffice. There is more material on thirteenth-century Valencian Jews than on Muslims, but the references are highly dispersed. Heretofore, scholars have formed some false impressions—for example, that Jews controlled court finances in the Crown of Aragon or that Valencian Jews were wholly urban—based on those relatively few documents that deal directly with Jews. Important but fleeting references in other documents correct such stereotypes. Burns devotes one chapter to problems such as these and another to vignettes of wealthy Jews from previously unnoticed families, pieced together from many documents. Two chapters are based on records of real-estate trials, an almost totally ignored documental genre. From this apparently dry source, Burns extracts numerous interesting themes, including the diffusion of Roman law at the grass-roots level, the relationship between town boundaries and grazing rights (a significant economic feature), and the social function of this kind of joint legal action, which served to induce solidarity among groups of newly arrived settlers.

THOMAS F. GLICK
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ALFRED HAVERKAMP. *Aufbruch und Gestaltung Deutschland, 1056–1273*. (Neue Deutsche Geschichte, number 2.) Munich: C. H. Beck. 1984. Pp. 356. DM 28.

The struggle for unification has long dominated German historiography. In the study of medieval Germany this has led to an emphasis on imperial history and an anachronistic preoccupation with the origins of the Bismarckian state. Classic examples of the genre are Karl Hampe's *Germany under the Salian and Hohenstaufen Emperors*, originally published in 1909 (translated in 1973), and, as the title suggests, Geoffrey Barraclough's *The Origins of Modern Germany* (1947). In recent years several scholars have attempted to break out of this mold. One example is Josef Fleckenstein's *Early Medieval Germany* (1978); Alfred Haverkamp's book, the second in a ten-volume history of Germany, is another. An expert on medieval Italy, Haverkamp places German history in its European context and incorporates the postwar findings of *Landesgeschichte* into his narrative. He portrays the two centuries from the accession of Henry IV to the election of Rudolph of Habsburg as the major period of change in medieval German history, during which the differences between Germany and its more economically and culturally advanced western and southern neighbors lessened and the basic structure of Germany was set for centuries.

The first third of the book is a survey of European history as it might be presented in an introductory course in medieval history. It is refreshing to read a history of the Salian-Hohenstaufen epoch that begins with an account of European expansion into the Mediterranean and that mentions William the Conqueror and Philip Augustus before Henry IV and Frederick Barbarossa. Significantly, Haverkamp refers to the Westernization rather than the

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Germanization of the east Elbian territories. The second and third sections discuss political, economic, religious, intellectual, and social developments before and after Barbarossa's election. One can inevitably question some interpretations or the omission of certain items. For instance, Haverkamp does not seem to be familiar with Karl Morrison's argument that Gregory VII did not restore Henry IV to the kingship at Canossa, and he ignores Saint Bernard's involvement in the Schism of 1130. There are also a few minor slips; for example, Dürnkrut is misplaced on the map. Yet on the whole Haverkamp has written an excellent, comprehensive, and highly readable synthesis of the most recent scholarship.

Nevertheless, I have some more fundamental reservations. The first section is in essence a preface. Haverkamp includes relatively few comparisons between Germany and its neighbors in the second and third parts and does not really explain why Germany was different. For instance, he states that feudal law aided central authority in France and England but worked to the advantage of the princes rather than of the emperor in Germany. This indicates, he comments, the ambivalence in feudal law (p. 239). I do not find this a satisfactory explanation. Second, although Haverkamp provides examples from all over medieval Germany, his perspective remains a national one. He mentions, for example, great territorial variations on the restructuring of the nobility (p. 288) but cites no specific examples, such as Heinz Dopsch's analysis of the differences between the social stuctures of the neighboring principalities of Salzburg and Styria. A section on a few representative territories, as F. R. H. Du Boulay presents in *Germany in the Later Middle Ages* (1983), might have given a better picture of the incredible diversity of German society. Finally, the old emphasis on the primacy of politics remains. Much of the discussion of the ministerialage, the towns, land clearance, and even the church still revolves around their relationship to the monarchy and its failure to use these institutions to its own advantage. In spite of these reservations, this book is a valuable contribution and should be translated into English.

JOHN B. FREED
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HEINZ ANGERMEIER. *Die Reichsreform, 1410–1555: Die Staatsproblematik in Deutschland zwischen Mittelalter und Gegenwart*. Munich: C. H. Beck. 1984. Pp. 334.

This chronological analysis and review of the discussions about reform of the empire is the fruit of a lifetime of research and publication by Heinz Angermeier. Although the book under review is in a sense the sequel to *Königtum und Landfriede* (1966), its primary significance is that it is nothing less than an analysis of the origins of the political constitution of German Central Europe that persisted until formal dissolution in 1806. Past scholarship has tended to see the imperial reform movement as an abortive effort by various addled German political figures to modernize the German state, which had been definitively sidetracked by the Reformation. Particularly when nationalist and statist interpretations of the German past were in vogue, the reform period was seen as a concatenation of disasters produced by "dreamers" who did not understand how a nation had to be built. Even though more recent writers, particularly students of estate institutions, have grown more sympathetic to the Old Reich (one need only mention Mack Walker and James A. Vann in the United States), many of the polemic valuations of earlier times continue to hang on for lack of detailed revision.

Angermeier argues that the effort to institutionalize public life in Germany was originally the work of the monarch and his supporters but that it soon generated a nonmonarchical countertradition supported by the princes. These princes, both ecclesiastic and lay, saw the Reich as a guarantee of their own liberties (Leonard Krieger's *Libertät*). Angermeier argues—against many scholars of the nineteenth century—that in the end the reform movement was played out between the princes and the monarch, with the other communities of the Reich (specifically, knights and towns) as spectators. After the mid-thirteenth century, he declares, the imperial towns and free towns played no meaningful role in German political life, least of all in imperial politics. Efforts to base the empire on mixed partisan federations such as the Swabian League proved sterile, as did efforts to place all of the monarch's powers in the hands of an executive council. The resolution to these failures was the institutionalization of the Reich as a juridical guarantor of the rights and privileges of sovereign authorities, an umbrella organization with no serious capacity to use its power aggressively or even defensively against those who would attack it.

Most surprising about Angermeier's study is that he carries it across the great divide of the Reformation and that the culmination of the movement came only with the Peace of Augsburg of 1555. The central institutions of the reform—the Imperial Chamber Court, the Imperial Diet, the Circles, the tax-quota (*Matrikel*), and the soon-defunct imperial government (*Reichsregiment*)—were already being tried out by the early 1520s, but their definitive shape came only in the middle of the sixteenth century. The Reformation in fact helped shape the Reich positively by reinforcing the secular autonomy of the emperor above both religious confessions of the German nation. Charles V comes

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through this discussion as a much more coherent German politician than he is usually credited to have been.

The footnotes will direct the reader to original sources, particularly the *Reichstagsakten*, whose middle series is now being completed; other references make clear that the Old Reich has become the subject of an active new scholarship in Germany now that the nation-state period is seen as an atypical episode in a history characterized by more complex polities. Angermeier does not repeat arguments he has made elsewhere in articles and his earlier book, so more advanced students will probably have to chase these references to get to the bottom of some questions. Angermeier's writing is clear and even enjoyable to read. There are two succinct sections on the intellectual propaganda that surrounded the imperial reform from Sigismund to Charles V. The author does not believe, however, that the work of publicists and theologians had much influence on the course of events; they generally followed the political trends, and at a considerable distance.

It is beyond my comprehension how a work of such complexity and richness could be published without even the most rudimentary index. This means that much of the learning contained in it will be beyond reach for many potential users.

STEVEN ROWAN
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JAMES H. OVERFIELD. *Humanism and Scholasticism in Late Medieval Germany*. Princeton: Princeton University Press. 1984. Pp. xviii, 344. $40.00.

This fine book challenges the opinion that in the German universities of the late Middle Ages the traditional scholastic culture and the new humanistic culture were locked in a bitter and irrepressible conflict. This view reflects the humanists' own complaints about persecution by obscurantist professors. But James H. Overfield has looked at a much broader range of contemporary evidence and has produced not only a fresh picture of intellectual life within the universities but also a picture of the process by which humanistic culture made its way into German life.

The book draws important conclusions: that humanists, despite their complaints about persecution, were present in all universities; that humanistic criticism of prevailing academic practices was much less frequent and much less total than was commonly thought; that the established scholastic leaders produced virtually no works that can be construed as antihumanist polemics; that the issue in the infamous Reuchlin affair was anti-Semitism rather than antihumanism; and that by the mid-1530s humanism had succeeded in transforming the university curriculum into something that was no longer scholasticism at all.

One conclusion that flows inevitably from the evidence is that German humanism was itself a product of the universities. Although the pioneers did study in Italy, German universities were the principal points of entry and diffusion for the new culture. Nearly all German humanists received their decisive initiation into humanism in northern universities. Yet, although humanists and scholastics coexisted, that coexistence was not always peaceful. Humanists' pressure for curricular change and for greater power generated constant tension.

It may seem ungracious to ask for more from so valuable a book. But, in assessing the relationship between traditionalists and reformers, the author should have emphasized the institutional pride and solidarity typical of late medieval universities, a major reason for the difficulties faced by academic reformers. Again, although it is true that the humanists did succeed in introducing substantial curricular reforms in the period 1515–35, Overfield is somewhat too optimistic in assessing the extent of that success. Erfurt, for example, did adopt some humanistic curricular reforms in 1518–19, but most of these reforms were dead letters because the university lacked the financial resources to put them into effect. And, although the author correctly observes that most academic reforms were imposed from outside by territorial princes, he does not make it sufficiently clear that, with a few exceptions, truly significant humanistic reform was a by-product of princely adoption of the Protestant Reformation.

Despite these caveats, this is a book that all persons who seek to understand the place of humanism in German culture must read. Humanism had to be brought into the mainstream of German life, and the book's chief service is its demonstration that, despite their conservatism and foot-dragging, the universities were the chief mechanism through which that change took place.

CHARLES G. NAUERT, JR.
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GILBERT DAGRON. *Constantinople imaginaire: Etudes sur le recueil des* Patria. (Bibliothèque Byzantine, Etudes, number 8.) Paris: Presses Universitaires de France. 1984. Pp. 358. 250 fr.

This book combines a traditional commentary on a set of texts with up-to-date *historie des mentalités*. It deals with the *Patria*, several middle Byzantine collections of curious stories about Constantinople and its monuments. For years, recognizing the many errors and distortions in the *Patria*, scholars inter-

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ested in the history of Constantinople have looked down on this genre. In his book Gilbert Dagron includes as an epigraph Alan Cameron's dismissal of the *Patria* in 1973 as "silly stories" (p. 5). But Dagron can now afford the quotation, for the *Patria* are enjoying a boom. They are increasingly quoted in the debate over the degree of continuity between the early and middle Byzantine periods, and recently Averil Cameron and four other scholars (Alan Cameron among them) have published a commentary with a translation of the principal text of the *Patria* under the title *Constantinople in the Early Eighth Century: The* Parastaseis Syntomoi Chronikai.

Dagron's book, which developed from a seminar he conducted at the Collège de France, consists of a survey of the development of the *Patria*; separate discussions of the texts' foundation legends, their accounts of philosophers who explain the city's marvels, and their stories of the Hippodrome; a translation of and commentary on another of the texts, the *Account of the Construction of St. Sophia*; and a few concluding remarks.

Dagron argues that the *Patria* show the middle Byzantine period in a particularly revealing light, refuting the overly facile judgment of most Byzantinists that Byzantium was a continuation of ancient Rome (pp. 13–19, 61–62). Rather, in the *Patria* we seen Constantinople "anemic and dilapidated," as it was after the seventh century (pp. 6–7), when the Constantinopolitans were "deprived of their history" (p. 19); about 750 a researcher "doubtless no longer had direct access to writers before the sixth century" (pp. 37–38). Dagron compares the mentality of the eighth-century Byzantine with that of an Algerian peasant who once told him that the personification of France on coins was "Madame Gascard"—evidently a deformation of "Madagascar" (p. 59). Although texts of the *Patria* after the eighth century avoid the most obvious absurdities, their spirit is essentially the same (p. 47).

Are these silly stories therefore the expression of a silly civilization? Dagron seems to imply that they are, but the example of the *Patria* hardly demonstrates it. Greeks of the fifth century B.C. had their myths of temples built by bees and of rivers that traveled from the Peloponnesus to Sicily; Renaissance Italians lent a ready ear to myths of the origins of Rome and explorers' tales of men with two heads; today in America many more people choose to read publications describing extraterrestrial visitors and infants born pregnant than open the *American Historical Review*. Although Byzantine scholars had and read more texts of ancient authors than we have, many who were not scholars, as always, found fiction more entertaining than truth. Dagron makes some useful remarks on texts that certainly have their interest, and he illuminates some aspects of the mentality of half-educated Byzantines. But the overall picture he gives of Byzantium in the middle period is misleading.

WARREN TREADGOLD
Hillsdale College

BENJAMIN Z. KEDAR. *Crusade and Mission: European Approaches toward the Muslims*. Princeton: Princeton University Press. 1984. Pp. xiii, 246. $26.50.

In this carefully nuanced study of Catholic attitudes toward Islam in the centuries before 1300, Benjamin Z. Kedar challenges the widely held assumption that crusade and mission were distinct and divergent responses by the Christian West to the Muslim world. He examines the paradox that in the period when the Catholic church converted Central Europe and much of Scandinavia no attempt was made to evangelize Islam. Kedar concludes that the church considered such missions futile because Muslim rulers punished attacks on their faith by death; consequently, Muslims were only able to become Catholics if they lived in Christian states. Many Muslims did so during the eleventh and twelfth centuries when, partly through crusading, Catholic rule was extended to much of Spain and also to Sicily and Outremer.

Yet the paradox remained that no systematic attempt was made to evangelize these conquered Muslims. Kedar only partially accounts for this: Christians, he argues rightly, were reluctant to allow their slaves to be baptized since this made them free men, in the Kingdom of Jerusalem at least, until Gregory IX abrogated that privilege. Other factors merit consideration, since most Muslims in these frontier states were not slaves but serfs, and, as Western society showed, baptism did not make a serf free. Penal economic consequences alone cannot account for the failure to evangelize these Muslim peasants; more important were the reluctance of Christian kings to alienate their labor force by interfering with its religious traditions, the unwillingness of most of the Catholic clergy to learn Arabic, and, in the Kingdom of Jerusalem where a Frank was defined as a "Christian of the law of Rome," a desire to preserve the distinction between conquerors and conquered.

A concern for converting Muslims developed slowly but received a great stimulus chiefly, though not exclusively, from the mendicant orders in the thirteenth century. Whereas some twelfth-century writers had considered crusading a hindrance to the evangelization of Islam, the thirteenth-century advocates of mission took a more guarded view. Frustrated by the refusal of Muslim rulers to allow Christians to preach, they commended the crusade as a means of opening up Islamic lands to Christian evangelization. Innocent IV endorsed this view of

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crusading, and this in turn influenced later crusaders, notably, Saint Louis.

Kedar examines with great clarity the complex relationship between crusade and mission at that time and in the process makes illuminating comments about various important thinkers, including Joachim of Fiore, Roger Bacon, Raymond Lull, and Saint Thomas Aquinas. On the penultimate page Kedar notes that Catholic missionaries who were allowed to preach freely in the Mongol empire converted few Muslims and observes that this calls into question the prevailing Western assumption that the bigotry of Muslim rulers was the principal obstacle to Islamic conversion.

A great merit of this work is that it poses many interesting questions while sustaining a convincing and important thesis. The author must also be congratulated for achieving a high degree of impartiality. Standing outside the Islamic and Catholic traditions, he draws attention to passages in the sources that, though accessible, are frequently neglected in the interests of ecumenism (for example, Dun Scotus's opinion that the children of nonbelievers should be forcibly baptized). This approach is essential to the formation of an honest historical perspective. The key texts are printed in the appendixes and, where necessary, have been re-edited.

BERNARD HAMILTON
University of Nottingham

HANS EBERHARD MAYER. *Mélanges sur l'histoire du Royaume Latin de Jérusalem.* (Mémoires de l'Académie des Inscriptions et Belles-Lettres, Institut de France, new series, number 5.) Paris: Imprimerie Nationale; distributed by Boccard, Paris. 1984. Pp. 163.

The two studies contained in this volume are separate but related contributions to the history of the Latin Kingdom of Jerusalem, for they both explore the evolving relations between the crown and its vassals. Stimulating and rich in insights, they offer new or heavily modified interpretations of a wide range of key episodes in the political life of twelfth-century Latin Syria. Of the two studies, the second, an analysis of the obligations of royal vassals to provide military service outside the kingdom, is meatier and more interesting. Hans Eberhard Mayer follows Jonathan Riley-Smith and Peter Edbury in tracing the development of vassalic duties as portrayed by the chroniclers—notably, Fulcher of Chartres and William of Tyre—against the background of claims made by the thirteenth-century baronial lawyers.

Mayer illustrates the difficulties that twelfth-century kings encountered in persuading their vassals to participate in campaigns either in northern Syria or against Fatimid Egypt. As a result, their activities were heavily circumscribed in both areas. Baldwin II faced opposition to campaigning around Antioch in 1119 and 1120; Fulk was forced to rely on household troops when in the north; and Baldwin III had to depend on Templar help in 1149 and abandon plans to assist the disintegrating county of Edessa in 1150. Just as restrictive or distortive of policy was the need to find money or land, either already existing or to be captured, with which to reward those vassals who accompanied Amalric to Egypt between 1164 and 1169. This placed serious obstacles in the path of Amalric's hopes of subordinating Egypt to Frankish control, most importantly by hindering the operation of the Latin-Byzantine alliance.

The author's conclusion that the kingdom's vassals were preoccupied with limiting their military obligations and acquiring new fiefs is hard to fault. His study demonstrates that John of Ibelin's juristic treatment of vassalic obligations (ca. 1265) was firmly rooted in twelfth-century practice. Its chief features were already in the process of formation during the reigns of Baldwin I and II and were fully present by at least 1170. This account thus goes some way toward reaffirming the value of thirteenth-century lawbooks as reflections of twelfth-century reality, as well as supporting recent arguments that royal authority in Jerusalem was severely limited from its earliest days.

Similar lessons can be drawn from the first study. This is a group of three essays about Baldwin I, and, although it lacks the coherence and some of the interest of the second study, it has much to offer. Mayer corroborates the traditional view that Baldwin was a ruler of exceptional energy and ruthlessness, giving the argument extra depth by a painstaking re-creation of Baldwin's activities before the First Crusade. Nevertheless, Mayer stresses the weight and effectiveness of vassalic opposition to Baldwin's controversial marriage to Adelaide of Sicily in 1113, writing that it brought about Baldwin's repudiation of Adelaide in 1117, with serious long-term consequences for the kingdom. In his analysis of this complicated sequence of events, as in his account of the succession crisis of 1117–18 and Baldwin's early life, the author guides the reader through limited and sometimes contradictory sources with admirable skill and meticulous scholarship.

NORMAN HOUSLEY
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MODERN EUROPE

IRVING SINGER. *The Nature of Love.* Volume 1, *Plato to Luther.* 2d ed. Chicago: University of Chicago Press. 1984. Pp. xiii, 381. $20.00.

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IRVING SINGER. *The Nature of Love.* Volume 2, *Courtly and Romantic.* Chicago: University of Chicago Press. 1984. Pp. xv, 513. $25.00.

These are the first two volumes of what is planned as a three-volume study of the theme of love in Western thought. The first (which in the present edition is a revised version of a 1966 original) begins its survey with Plato, Aristotle, Plotinus, Ovid, and Lucretius and then goes on to discuss the Christian idea of love in the Middle Ages by focusing on four versions of it: eros (longing for God), philia (an idea that links both human fellowship and union with God), nomos (obedience and dedication to God), and agape (God's own spontaneously generous love). The second volume discusses the stages by which religious love was humanized and sexual love idealized in the Middle Ages, the Renaissance, and the modern periods. The work is to be completed by a third volume that will bring the study up to the twentieth century and should also tie the themes explored in the more specifically historical studies of the first two volumes to Irving Singer's own philosophical analysis of the nature of love.

Some effort is made at the beginning of the first volume to sketch a theoretical framework for the latter purpose. This is cast in terms of a contrast between two opposing conceptions of love that Singer calls "appraisal" and "bestowal" and that he says he has chosen "because I think they may be useful in constructing a new philosophy of love" (vol. 1, p. 44). "Appraisal" refers to the valuing of qualities discovered in or attributed to the beloved. The meaning of "bestowal," however, seems less clear. It is described as an imaginative relation that treats the beloved "as if" he or she had qualities worthy of appreciation, but with a continuing awareness of the fictitiousness of those qualities; Singer likens this to a theatergoer's willing suspension of disbelief. His purpose in defining these concepts as opposing extremes is to set up a framework within which to explore the theme of "idealization," that is, imaginative transformation of reality, as the basic dynamism of love. Singer makes quite clear that he favors the model of bestowal rather than that of appraisal, although these volumes do not make clear exactly why one should. How successfully he will work this out remains for the final volume to demonstrate.

Whatever one's attitude toward the theoretical framework, for most readers the major interest of these volumes will probably lie in their historical treatment of the theme of love from Plato to the present. For this they are recommended highly, not only because they are unique in offering so extensive a survey of this theme among a large number of major thinkers but also because Singer has studied his sources in depth and presents penetrating and often brilliant analyses of them that correct and greatly improve on those of such earlier scholars as Denis de Rougemont and C. S. Lewis. Both volumes are filled with insights that illuminate connections between historical configurations that may have seemed sequestered from each other in different streams of tradition. In fact, one of the major values of this study is the way the author places religion, literature, and philosophy in relation to one another under the overarching theme of love. Singer casts a new light on such figures as Descartes, Spinoza, Hume, Rousseau, and Hegel by taking them out of the more customary context of epistemological and metaphysical problematics and considering them as philosophers of love. This he also does in the case of Luther—a religious thinker—by interpreting his thought as precursory, through the ironic twists of history, to the idea of romantic love.

EUGENE WEBB
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JEAN DELUMEAU. *Le péché et la peur: La culpabilisation en Occident, XIII^e–XVIII^e siècles.* Paris: Fayard. 1983. Pp. 741.

This is a work of synthesis with a thesis. Stating it most succinctly in the conclusion but developing it with relentless purpose throughout the 627 pages of text, Jean Delumeau argues that from 1348 to 1648 a mentality of sin and fear dominated both Catholic and Protestant Europe. In the book's three sections—"Macabre et pessimisme de la renaissance" (p. 15–208), "Faillité de la rédemption?" (pp. 211–363), and "La postorale de la peur" (pp. 369–547)—Delumeau explores art, literature, theology, sermons, and pastoral care to discover the core of this mentality among the "classes dirigeantes" and to document its diffusion.

At the remote origins are the Bible and classical tradition, then the desert Fathers, monks, and mystics. But Saint Augustine is by far the most influential and authoritative teacher of an ascetic mentality in which the earth is a vale of tears, few are saved, human capability is slight, and human culpability is enormous. In the fourteenth century a macabre spirit heightened fear of sin and revulsion against sexuality. It is reminiscent of the Middle Ages of John Addington Symonds, but without his Renaissance, for in Delumeau's synthesis both Renaissance and Reformation carry on in the gloomy tradition. What is crucial about the period 1348–1648 is the popularization of this complex, so that commonplaces of monastic asceticism provide the dominant themes of the European mentality.

Delumeau's dating of this adoption of an ascetic mentality by wider and wider circles of European laity signals the causality he finds behind it. It was

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not laid down in Scripture; it was not an inevitable product of the Christian tradition. On the contrary, a coincidental series of collective disasters stretching from the Black Death to the end of the Wars of Religion (with a familiar litany of schism, international and civil war, peasants' revolts, and continued assaults of epidemic disease) provided the social and psychological conditions that make the strict linkage between catastrophe and sin plausible. One might have thought, he conjectures, that the siege mentality, developed in this period and documented in his *La peur en Occident* (1978), would have been satisfied with its external enemies: Turks, idolators, witches, Jews, and heretics. On the contrary, the collective experience of disaster made the West receptive to the messages that plague, syphilis, witchcraft, and melancholy (to name but a few) are linked with sin (pp. 152–58). "No civilization has ever accorded so much importance—or value—to guilt and shame as the West from the thirteenth to the eighteenth century" (p. 10): that proposition Delumeau defends with massive evidence taken from the confessional (pp. 211–65), the preparation for death (pp. 389–415), and, in all of part 3, from pastoral works that preached these messages. Sin and fear dominated clerical thinking and pastoral care even beyond 1648 (see the interesting quantitative analysis on pages 475–97 and 536–47), and this Christian strategy explains the pessimism and melancholy of the early modern period as well as the weakening of Christianity in our own day.

This is a powerful and massively researched work. It puts secondary literature to good use and retains respect for the language of its primary sources, which it quotes voluminously. It is not heavy-handed, censorious, or doctrinaire. Delumeau tells us that he believes in understanding, not judging, the past. He says he believes in "sin" and sees some philosophy of the imperfection of human nature as simple common sense. Finally, he believes that our own age is hardly one to feel superior about accusation and repression (p. 11). Throughout the work he recalls, if only briefly, evidence of other moods in the era. He often reminds us of the nuances in the material he is mining. If his thesis is stark, Delumeau believes he has arrived at it after careful consideration of the contrary evidence and arguments.

In this short review the variety of subjects touched on in this wide-ranging synthesis is too vast even to be suggested by sampling. Poets, playwrights, painters, novelists, Protestant and Catholic theologians, preachers and laypeople, Catholic Reformation Spaniards, and New England Puritans—such a list gives only a hint of Delumeau's scope. And, even on subjects one knows rather well, one is likely to find suggestive ideas and bibliographic references.

But in a territory so vast, it is also inevitable that one sees gaps in argument, interpretation, and bibliography. For example, "The Custom of the Island of Cea" does not give rape as a reasonable cause for a woman to commit suicide (pp. 206–07)—Montaigne's argument is quite the opposite. In the "First Anniversary" Donne may have drawn on the language of the *contemptus mundi* (p. 172), but he did so to construct a conceit whose point is totally different from Delumeau's. Chaucer may have written and meant every word in the "Priest's Tale," but only a very complaisant reader will allow Delumeau to ignore the rest of *Canterbury Tales* (pp. 231–32). The magisterial reformers may have spoken a medieval language of guilt and fear (pp. 33–39, 563–623), but they also believed that they had a consoling theology of justification; Luther in particular expected it to make the faithful happy. Similarly, laxism in the moral and sacramental theology of penance finds exponents as early as the thirteenth century and continues as a legitimate current after the Reformation (as Delumeau has himself shown in *Catholicism from Luther to Voltaire* [1977]). And, though it may seem petty to criticize an author who has read as much as Delumeau, I feel compelled to insist that a discussion of Renaissance anthropology without a thorough appreciation of the work of Charles Trinkaus, especially *In Our Image and Likeness* (1970), cannot be convincing.

Yet none of these criticisms (even the last) should discourage a reader or offer an excuse to pass up this book. An old thesis about medieval Christinaity has been revived by an imaginative historian, applied to a longer period, and given more compelling form. For that "vue d'ensemble" and for its fascinating individual segments, *Le péché et la peur* deserves a thoughtful, if critical, reading.

THOMAS N. TENTLER
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PAOLO ROSSI. *The Dark Abyss of Time: The History of the Earth and the History of Nations from Hooke to Vico.* Translated by LYDIA G. COCHRANE. Chicago: University of Chicago Press. 1984. Pp. xvi, 338. $35.00.

In the 1960s a number of historians of science took the position that science had its own internal logic of development independent of historical context. Each successful step pointed toward the next problem. Looking backward from modern science makes it easy to see who took the right steps; from this presentist point of view, why waste time on the defenders of lost causes? Paolo Rossi responds to that question by reconstituting for the century between 1650 and 1750 the context of values and beliefs that influenced the perception of significance and truth on problems of the origins of the earth, mankind, language, and culture. He emphasizes the

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history part of the history of science, and he finds no clear winners and losers.

One of the central themes Rossi addresses on the matter of origins is overcoming the biblical scale of historical time. In the first part of his book, "The Earth, Time, and Shells," he gives a detailed review of the challenge of geology to the chronology and history of the earth based on the Scriptures, ending with an examination of the vast scale of time proposed by Buffon, whose phrase "the dark abyss of time" Rossi used in his title. What Rossi aims to elicit from this account is not just the steps on the road to geological science but also the kind of reasoning and prepossessions that entered into the defenses of received views against challenges such as fossil evidence. He traces the paths of accommodation and transformation between old beliefs and new challenges of geological evidence and draws out their implications for a context of ideas and beliefs beyond scientific factuality. He also points to the contributions to geological theory of some who tried to integrate new geological evidence with established belief in nonscientific systems and cosmic romances.

In part 2, "The Ages of Human History," Rossi turns to parallel challenges to biblical chronology and history on the issue of the origins of the human race and culture. These challenges came from rationalists, and the issue was real history versus fabulous history. In part 3, "Barbarism and Language," Rossi carries the debate between biblical history and profane history to the problem of the origins of language. Again, Rossi seeks to uncover in these debates what the age saw as the significant questions and how the debates set the stage for later linguistic theory.

One of the author's interests in reviewing these debates on time and origins is to present the contextual background of Giambattista Vico's work. Rossi shows in the course of his work that many of the claims for Vico's modernity have been made without an awareness of the background of debate on questions that Vico tried to answer. Yet the main accomplishment of Rossi's historical reconstruction is to provide an important illustration of the complexity that underlies changing perceptions from one age to another.

FRANCIS C. HABER
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MARIO A. DI GREGORIO. *T. H. Huxley's Place in Natural Science*. New Haven: Yale University Press. 1984. Pp. xxi, 253. $25.00.

Thomas Henry Huxley was more than "Darwin's bulldog." During the high Victorian age he was science's most visible public spokesman and one of the most eloquent it has ever had. The elegance of Huxley's essays and addresses has led to his being more appreciated by literary scholars than by historians or scientists, yet he spoke with authority for science because of a reputation built on a lifelong commitment to scientific research. Huxley's science has received little attention from historians, and, although Mario A. di Gregorio's book falls short of filling the gap, it does suggest some reasons for the long neglect.

Huxley established his reputation as a young invertebrate morphologist. He became a vertebrate paleontologist out of the necessity of earning his livelihood through science, and, once his reputation was secure, he also turned to the controversial problems of physical anthropology. To each of these fields he made solid, respectable contributions including some (such as the reptilian origin of birds) of lasting scientific importance. But, unlike his public persona, Huxley the scientist was a conventional, even conservative, thinker. He might challenge an established authority such as Richard Owen, but, as di Gregorio correctly argues, he was a rebel, never a radical, in science. He was, in fact, an accomplished practitioner of what Thomas Kuhn has called "normal science," which perhaps accounts for both his scientific eminence in his own time and his eclipse in ours.

One of the virtues of di Gregorio's book is its attempt to take Huxley's science out of Darwin's shadow. Still, most readers will find most interesting the treatment of Huxley's ambivalence toward evolution. Among the topics di Gregorio discusses are Huxley's commitment to Karl Ernst von Baer's nonevolutionary type concept, his reluctance to include evolution in his own work for nearly ten years after the publication of the *Origin of Species*, his disagreements with Darwin over natural selection and the gradualness of evolutionary change, and his differences with Darwin over scientific method. This last point is particularly significant and revealing. Like some modern-day creationists, Huxley wanted a decisive experiment, some empirical proof of the evolution of new species. He was never content with Darwin's more radical reliance on the broad explanatory power of the theory even in the face of the impossibility of such an *experimentum crucis*. Unfortunately, di Gregorio's detailed descriptions of scientific issues lack a comparable historical analysis and are so laden with jargon that they will be intelligible only to trained specialists.

The real problem with the book, however, lies less in its thickets of jargon, or even its uncertain organization, than in di Gregorio's self-imposed tunnel vision. Although conceding that "Huxley acted most decisively as a *scientist* when his *extrascientific* concerns were also at issue" (p. 190), di Gregorio concentrates exclusively on the science. The result is that we have Huxley's science without Huxley and with little

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historical context. We are given detailed accounts of Huxley's scientific accomplishments, but no real understanding of their significance. At times, di Gregorio does attempt to explain Huxley's actions in terms of his character and attitudes, but he provides little insight into either. The final chapter is an almost separate essay on Huxley's character, but it, too, is a statement of conclusions without the evidence or reasoning on which they were based. It left this reviewer with the uneasy feeling that di Gregorio knows and understands much about Huxley but that somehow this understanding was not allowed to enlighten or enliven this book.

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ELISABETH CRAWFORD. *The Beginnings of the Nobel Institution: The Science Prizes, 1901–1915*. New York: Cambridge University Press or the Editions de la Maison des Sciences de l'Homme, Paris. Pp. ix, 281. $34.50.

One of the sure signs of autumn is the annual awarding of the Nobel prizes, which are the most prestigious and lucrative of all prizes in the sciences. Media interest in the event and the prize winners is high, and we assume it has always been this way. But such is not the case.

Elisabeth Crawford traces the establishment of the Nobel Foundation and the awards, from the drawing up of Alfred Nobel's will in 1895 to the suspension of the prizes in 1915 because of World War I. Concentrating on the science prizes, she places the foundation's institutions and their function in the context of Swedish political and cultural life and the prizes in that of the history of science prizes in general.

The section in Nobel's will establishing the prizes was one paragraph long; the criteria for the awards and the institutes involved were only briefly identified. The job of implementing Nobel's will fell initially on the executors, and they understood the implications of his intent. For the first time truly international prizes that involved unprecedented amounts of money were established. Thus, the executors drew the Swedish academies and government into the initial negotiations that established the foundation, and from the beginning the prizes were enmeshed both in national politics and in the politics of the Swedish scientific community.

The next step was the academies'. The conduct of the award committees determined whether annually the academies merely donated large sums of money to worthy individuals or established a culturally and politically significant tradition. Despite political factions and compromise choices made under conflicting pressures, they established such a tradition owing to their high-mindedness and hard work.

Crawford contrasts the inner dynamics of the two committees and nicely details the importance of a strong chair and the pivotal influence of individual committee members. These committees emerge as the most influential groups in selecting recipients of the prizes. Despite the provisions of Nobel's will, by World War I prizes were awarded exclusively for esoteric research.

This spotlights the difference between Nobel's vision of the prize, shared with the public and the press, and what scientists made of it. The prizes were always important press items. The amount of money involved made the awards newsworthy and the winners public figures. But only gradually did the prizes attain any stature in the scientific world and become worthy of extended comment in scientific journals. Although the money represented significant research funds, the prizes did not carry the prestige in the sciences that they do today.

This is science clearly in its social context. There is a welcome absense of ideology and formal sociological method and structure. The narrative carries this fascinating story. Although this is not a biography, the author could have given us a little more on Alfred Nobel, who remains an enigma. And one must quarrel with her interpretation of mainstream physics in this era. The historian who is not a specialist should beware, for Crawford extrapolates her conclusions about Swedish physicists to physicists in general. This is simply not supported by the evidence and is not necessary to her argument. The account is straighforward, and not one word is superfluous. Therefore, in order to follow the account of the establishment of the prize system, the reader is advised to read, not skim, the appendixes on Nobel's will and the statutes of the Nobel Foundation.

This book demonstrates how effectively scientists adopt existing political and cultural forms to accomplish their own goals. It will be useful for historians trying to integrate the sciences into Western civilization and historians of science, for it will contribute to their discussions of issues surrounding the examination of twentieth-century institutions.

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GERALD D. FELDMAN *et al.*, editors. *Die Erfahrung der Inflation im internationalen Zusammenhang und Vergleich/The Experience of Inflation: International and Comparative Studies*. (Veröffentlichungen der Historischen Kommission zu Berlin, number 57; Beiträge zu Inflation und Wiederaufbau in Deutsch-

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land und Europa, 1914–1924, number 2.) New York: Walter de Gruyter. 1984. Pp. xv, 426. DM 98.

Perhaps one of the most successful recent ventures in international historical research cooperation has been the project "Inflation and Reconstruction in Germany and Europe, 1914–1924," initiated by Gerald D. Feldman at the *Historische Kommission zu Berlin* in 1976 and later supported by the *Volkswagen Stiftung*. The project is reaping a considerable harvest of articles and monographs and, above all, of volumes that are the result of team work directed by a steering committee whose members function also as editors (Feldman, Carl Ludwig Holtfrerich, Gerhard A. Ritter, Peter-Christian Witt). The first volume focused on government economic policy and the social and political aspects of the inflation in Germany. The second, which is reviewed here, extends the inquiry into comparative history chronologically, geographically, and thematically.

The contributions are divided into three parts, each of which is introduced by a rapporteur, and this gives the volume the required cohesion it otherwise might have lacked. In addition, each of the eleven essays (four are in German) has a summary in English. This arrangement makes the book easy to survey, but the selective reader would have found an index of names, places, and subjects helpful.

In part 1 approaches to and interpretations of the postwar inflations are debated. It clearly emerges that, although the economic theories and quantitative models for assessing past situations can constitute an important tool and can explain certain isolated phenomena, these methods alone do not suffice to elucidate complex historical realities. Any serious approach to economic history must take into account varying sets of circumstances that change in time and space. In relation to the German case Kindleberger argues that "much of the technical literature on the German inflation is beside the point because it fails to specify the socio-political matrix in which economic events took place" (p. 31). Why Kindleberger calls his view "structural" is not quite clear, for his approach is more precisely multicausal—an approach near to the heart of the historian. In contrast, Sargent subordinates four different inflations (in Austria, Hungary, Germany, and Poland) to one model and comes up with the simple answer that all inflations can be ended by a quick and drastic shift in the "policy regime." This conclusion he reaches all the more easily by concentrating on monetary matters and by ignoring everything else, including the results of recent research. How untenable such a monocausal explanation becomes is shown by Maier's comparative assessment of the inflations and stabilizations after both world wars. Although it seems that the United States government learned the lessons of the disastrous aftermath of the First World War and assumed greater responsibilities in the stabilization of capitalist Europe after 1945, the 1970s ushered in an international economic destabilization that was fraught with danger.

Parts 2 and 3 contain studies based on thorough empirical research and afford new insights into the process of inflations of the 1920s in Europe. The East Central European experience is the subject of part 2. Kernbauer and Weber trace the largely negative impact of the breakup of the Habsburg monarchy and the rampant inflation on the leading Vienna commercial banks, which led to their internationalization, on the one hand, and to their multinationalization in the Danubian area, on the other. Elizabeth Boross analyzes the origin and activities of the State Issuing Bank during the inflation in Hungary. Unvalorized credits by this bank to private business exceeded those made available to the state by about 50 percent in 1923. Her argument that the State Issuing Bank became a potent source of hyperinflation is substantiated by solid and previously unknown archival evidence. A truly comparative study is presented by Bloomfield, who deals with foreign trade in the "harsh world" of the postwar inflation in Austria and Czechoslovakia. Both countries had lost access to a large home market and became excessively dependent on exporting their produce. His detailed statistical analysis within the complex historical framework of the two successor states highlights the sharp upturn in exports that both experienced as their currencies depreciated. But the Czechoslovak state quickly brought inflation under control without seriously distorting the economy. Austrian exports, however, showed a rise in raw materials and a fall in manufactured goods, an unfavorable trend that also continued after the hyperinflation and negatively affected industrial production. The currencies of Austria and Hungary were stabilized with loans from the League of Nations, but Poland's finance minister attempted to stem the tide of inflation between 1923 and 1925 without foreign aid. According to Landau and Tomaszewski, the Grabski Plan did get off the ground but ultimately failed, and after Pilsudski's *coup d'état* American aid and control had to be resorted to in a second bout of inflation in 1927. The authors connect the refusal of League of Nations aid with Polish nationalism but say little about the background of competition between the British attempt to create a network of central banks in Europe and the American and French efforts to gain control.

The last part compares post–World War I conflicts in German and British industrial relations in the chemical and engineering industries (in essays by, respectively, Patton and Wendt) and investigates the problem of the eight-hour day in the German

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and American iron and steel industries (in a chapter by Irmgard Steinisch). Although strikes, lockouts, and other confrontations coincided with inflationary developments, the mutual influences were tenuous. There is no doubt that more empirical studies of this nature are needed to permit more valid general conclusions.

This volume is to be welcomed as a substantial step forward in explaining the socioeconomic and political context in which inflations took place. It is, however, doubtful whether the findings of the research project can, in the words of Feldman, "make more intelligent and informed action [by governments] possible" (p. viii), but they can, by deepening our historical knowledge, contribute to a better understanding of the present.

ALICE TEICHOVA
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LORNA S. JAFFE. *The Decision to Disarm Germany: British Policy towards Postwar German Disarmament, 1914–1919*. Boston: Allen and Unwin. 1985. Pp. xiii, 286. $29.95.

Lorna S. Jaffe's book is an important contribution to modern European history. Hitherto, little scholarly attention has been devoted to tracing the origins of the British attitude toward German disarmament at the Paris Peace Conference of 1919. Through the exhaustive use of British papers in public and private hands, Jaffe has documented the story behind David Lloyd George's insistence on Germany's disarmament at the peace conference. She shows that the prime minister's demand reflected the interrelationship between domestic and imperial politics, on the one hand, and foreign and defense policy, on the other. One of her most telling points is that the disarmament of Germany was not one of Britain's war aims but became a panacea at the end of the war for a variety of problems Lloyd George and his government faced.

In the first part of her study Jaffe points out that the war initially provoked in Britain a condemnation of Prussian militarism and calls for its destruction. This passed as the conflagration lingered. Thoughts of disarming Germany remained unrealistic until its spectacular collapse in the autumn of 1918, but by that time Britain had already formulated its war aims, which did not include German disarmament. The terms of the armistice ensured that the German army posed no threat to the peace of Europe in the foreseeable future, and the British government feared that to weaken Germany much more would expose her to bolshevism. As Jaffe writes, "Consequently, despite the Coalition's campaign position, between the Armistice and general election members of the British Government expressed no interest in further disarming Germany" (p. 119). Nevertheless, on the eve of the election, Lloyd George, in an attempt to assuage the growing demand for demobilizing the British army, linked that issue with the abolition of conscription in Germany. "With the Coalition's victory at the polls, Germany disarmament was well on the way to becoming an important part of Britain's peace policy" (p. 141).

Lloyd George next concluded that by embracing German disarmament, carried out through the League of Nations, he would win American support for British objectives at the peace conference as well as gain an ally in thwarting French demands for the dismemberment of Germany. A disarmed Germany also offered the prospect of general European disarmament, which would eliminate the need for a long-term British commitment to the Continent. At Paris the British therefore approached the question of German disarmament from the point of view of solving their own domestic problems rather than considering Germany's likely reaction to forced disarmament.

In her conclusion Jaffe is severely critical of the shortsightedness of British policy. To be sure, the final disarmament clauses did ensure the survival of the Prussian mentality within the German army, but Britain's willingness to condone violations of the Versailles treaty had equally fatal consequences. "British policy makers' failure to examine the assumptions upon which they based their policy led them to pursue a course of action which undermined the European stability and peace which they were determined to preserve" (p. 224). Although that may be too strong a statement for some, scholars in modern European history will welcome Jaffe's valuable study of this important but neglected aspect of the Treaty of Versailles.

F. RUSSELL BRYANT
University of Alabama

MORTON H. COWDEN. *Russian Bolshevism and British Labor, 1917–1921*. (East European Monographs, number 63.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press. New York. 1984. Pp. viii, 238. $25.00.

This book examines Bolshevik relations with the British labor movement in the period after the Russian Revolution. Drawing principally on the Soviet press, it traces widely fluctuating estimates of the revolutionary potential of Britain. Soviet commentators, from the most sanguine to the most cautious, shared a propensity for exaggerated responses. Since every encouraging development, from the unrest at the end of the war to the Council of Action in 1920, took on a heightened significance, so the persistent moderation of the Labour party

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and union leadership triggered angry recriminations. The British working class was alternatively congratulated for its imminent revolutionary achievement and chided for its incorrigible backwardness.

Morton H. Cowden conveys the flavor of these estimates with extensive quotations. He is more convincing on the Russian end than on the British, and the somewhat breathless narrative leaves insufficient opportunity for analysis. His writing is most successful when he gives himself room to discuss differences of opinion, as in the chapter on the Second Congress of the Second International. The book also needs a more sustained treatment of sources of information about British events. Clearly, Lenin was not alone in his misunderstanding of the significance of groups and individuals, and one wants to know more about the basis of these estimates. Did they rest on left-wing sources alone, or were they supplemented by the British press?

In an effort to vivify his study the author strives for the colorful phrase. Engels sneers in the opening pages and Lenin watches smugly; Litvinov becomes a "whirling Bolshevik dervish" (p. 19), and Sylvia Pankhurst, who appears here on most occasions as Miss Pankhurst, is accorded a woman's prerogative to be late. The conclusion advances the bizarre proposition that Washington and London, having learned from this book that communism is not monolithic, might simulate Kremlin debates in order to take advantage of their longueurs. These blemishes should not distract readers from a useful monograph.

STUART MACINTYRE
University of Melbourne

WILLIAMSON MURRAY. *The Change in the European Balance of Power, 1938–1939: The Path to Ruin.* Princeton: Princeton University Press. 1984. Pp. xix, 494. Cloth $50.00, paper $19.50.

After providing four background chapters on the resources and perspectives of the principal powers, Williamson Murray monitors the crescendo of international crises between the Anschluss and the outbreak of the phoney war in the West. He does so with an expertise acquired from work in English, German, and American archives. What interests him most is the military side of these crises: the strategic planning, the tactical doctrines, the economic resources for rearmament, and the political role of military advisers. The German military is his particular strength, as is evidenced by his valuable exposition of the military operation in Austria, the plans and deployment for the autumn of 1938, and the implementation of Hitler's war plans one year later.

But how novel are his conclusions? Murray reveals nothing new on the controversies about Hitler, who wanted mastery of Europe and to that end was prepared to risk a general war anytime in 1938–39. Nor does Murray include anything new on appeasement or the balance of power. Indeed, these are but occasional expressions, whose meanings are taken to be self-evident. Newer, at least in terms of vigorous argumentation, is the idea that Germany would have been defeated in 1938 and, therefore, that Hitler's decision to negotiate resulted in self-induced misfortune for the Allies. Newer still in tone and singular in emphasis is the severe indictment of Neville Chamberlain, from which even the French emerge with some credit.

This judgmental quality is characteristic of the book, in which almost no one appears in a good light. Every chapter is littered with the branding vocabulary of "failure," "misperception," "mistake," and "error," from which hastens the descent to "blame," "right," and "wrong." Not until the conclusion do we learn why, for only there does Murray unveil his theory of "incompetence as the basis of human behaviour" (p. 359), some kind of Murphy's Law in earnest. Suddenly, all of the earlier signs of intemperance come together in a few withering pages on German strategic "incompetence" (p. 360), the West's "ignorance" of strategic affairs (p. 367), the "bad" statesmanship (p. 357), the "faults" in British policy (p. 364), and the myriad "failures" of the West (p. 362), the Chamberlain government (p. 363), the French air force (p. 365), the French government (p. 365), and Allied policy (p. 367).

The common cause of these historically inspired jeremiads is striking. Caution us though he does that the men of the 1930s could only read their enemies' intentions "dimly and imperfectly" (p. 385), Murray is incredulous at the "failure" to see that war was "inevitable" (pp. 283, 296, 362). In short, he dismisses his own counsel, so sure he is of what happened. Thus, like some Tyburn magistrate he dispenses a rough and rapid justice to all in his court. Predictably, too, some of these hastily pronounced verdicts seem spectacularly subject to appeal—whether they concern the alleged failure of the British government to shake off its "profound apathy" during the Abyssinian crisis (p. 72), the purported refusal of French politicians to take the air menace seriously until the late 1930s (p. 109), or the attribution of direct responsibility to Léon Blum and the Popular Front for the Munich humiliation (p. 364).

Still, the provocative quality of this work, together with its information and insights on military matters, help spare this book from the author's own theory of human behavior. This, one can only suspect, will not surprise him.

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MIRJAM KÖLLING. *Führungsmacht in Westeuropa? Grossbritanniens Anspruch und Scheitern, 1944–1950.* (Studien zur Geschichte, number 3.) Berlin: Akademie der Wissenschaften der DDR, Zentralinstitut für Geschichte. 1984. Pp. 223.

The political ambience of communist countries is likely to reduce writing on recent history to what Marxist apologists call vulgar Marxism. This features facile assumptions about economic systems and their determining influence on behavior of protagonists, who, if they are not among "progressive forces," are allowed no tragic dignity. What fellow feeling and pity should be spared for those who, blocking the march to human liberation, are doomed to history's rubbish heap?

This third volume in the Historical Studies series published by the Central Institute for History in the Academy of Sciences of the German Democratic Republic is a widely researched and skillfully composed study, whose inadequacies are rooted in vulgar Marxism and the use of explanatory and determining terms—like "ruling classes," "imperialism," "monopoly capitalism," "bourgeois," "progressive forces"—that are not established by evidence and illustration or clarified by analysis. A shallow view of motivation and, above all, a restricted context distort Mirjam Kölling's story into a moralizing and psychologically unconvincing account of the failure of imperialist Britain's bid to lead Western Europe from 1944 to 1950.

Its power declining, Britain followed an anti-Soviet policy in the last period of the war without changing direction when Ernest Bevin, the ardently anticommunist trade union leader, became Britain's foreign secretary. In seeking the leadership of Western Europe, Bevin hoped to add to Britain's waning power and direct the resources of Europe in an anti-Soviet coalition. Implicit in such goals were the permanent division of Germany and West German rearmament. Kölling's narrative points out the irony that, although Britain's West European policy had some effect on, for example, the Treaty of Dunkirk, at the very same time financial pressure compelled Britain to withdraw from Greece and Turkey. Freed of that burden, "British imperialism could devote itself all the more vigorously to the task of leadership in Western Europe" (p. 114). But Washington's "lightning quick reaction"—the Truman Doctrine—"made manifest that it then stood at the ready to assume Great Britain's role as counter-revolutionary vanguard" (p. 116).

Kölling presents Britain and the United States as single-minded counterrevolutionary warmongers by not describing Soviet interests, policies, and actions. The Soviet Union was building socialism and a peaceful world at home and in Eastern Europe, where it faced the bad faith and hostility of the British and the Americans. The Soviet Union was merely acted on; it was not one of a number of states interacting, with varying degrees of knowledge and suspicion. Kölling tells her story in the manner of one who summarizes *King Lear* by omitting all details of Lear's division of his kingdom between two daughters with whom he and his men would alternately stay. To limit Lear's story to his subsequent behavior and ranting is to present him as the royal burden and persistent victimizer of his daughters.

The author cites material from unpublished Foreign Office documents. Her quotations from Commons' debates and newspapers are secondhand. She cites a considerable range of English works, some French memoirs, and East German studies. Alan Bullock's third volume on Bevin appeared too late for her to use, but it is questionable whether the author would have been influenced by it. There is no evidence that Kölling confronted intellectually any thesis counter to her own. She evidently used other works as sources of quotations favorable to her immediate point. Where she used writers struggling to be fair, she found rich pickings.

M. A. FITZSIMONS
University of Notre Dame

M. L. BUSH. *The English Aristocracy: A Comparative Synthesis.* Dover, N.H.: Manchester University Press. 1984. Pp. vii, 248. $27.00.

Recent historical study has probably done more with Europe's peasantry than with Europe's aristocracy or middle class. As popular political preferences move to the right, however, this seems to be changing. One sign is the work of M. L. Bush. He has already written an ambitious book on European nobility, dealing with their hereditary privileges, and he plans a series of companion volumes "to explore more fully the other salient characteristics of the European nobility, especially its changing ideals, composition, wealth and power." The volume under review is not one of this series but instead a more limited study, confined to the English aristocracy. But even Bush's limited study, it would seem, outdoes what a Lawrence Stone, an H. J. Habakkuk, or an F. M. L. Thompson have hitherto done. They have been content (except for Stone in his most recent work) to study the English aristocracy for a period of a century or so. Bush is content with nothing less than a complete history from the Middle Ages to the present. And for good measure he puts it all into a comparative context of Continental aristocracies.

The main conclusion of this unusual book is a negative one: that historians of England's aristocracy have overemphasized "its alleged oddities" and underplayed its having "much in common with the

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continental nobilities" (p. 1). This may be true, but not because English historians have been serious comparative historians. Their detailed national work has forbidden that. If they have overdone the oddities of the English aristocracy, theirs has been a marginal, for the most part implicit, statement. Surprisingly, despite his thesis, Bush himself has much to say about English oddities, and his case is not helped by observing that "the English aristocracy possessed significant peculiarities, but so did every European nobility" (p. 6).

Nor is it helped by a proneness to inaccuracy. For example, did aristocratic membership in the House of Commons fall from 74 percent to 32 percent between 1865 and 1874 (pp. 151–52)? Was David Lloyd George's supertax "a special tax on the landed" (p. 156)? Nevertheless, Bush has done well to invite national historians into the larger realm of comparative history and to introduce them (if they need the introduction) to Continental secondary work. Comparative study certainly needs doing, although progress will not be easy and rapid. We are still at a primitive stage where neither questions for comparison nor the possibility of answering them (in terms of available archives) are altogether clear.

DAVID SPRING
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JO ANN HOEPPNER MORAN. *The Growth of English Schooling, 1340–1548: Learning, Literacy, and Laicization in Pre-Reformation York Diocese*. Princeton: Princeton University Press. 1985. Pp. xix, 326. $40.00.

By examining the evidence of educational activity in the diocese of York in the two centuries before the Reformation, Jo Ann Hoeppner Moran demonstrates that the much-touted "educational revolution" of Elizabethan and early Stuart England had vital roots in the medieval period. The sixteenth century, rather than being an age of new and dramatic developments in education, can now be seen as one of continuity, building on the growing momentum of a well-established and expanding medieval structure.

The period 1340–1548, we are told, saw "dramatic educational development" and an "explosion of grammar education" (pp. xiv, 221). The schools of this northern diocese in the later Middle Ages had "far greater variety and more remarkable vitality than previously suspected" (p. 91). "Opportunities in elementary and secondary education before 1548 were much greater and expanding more rapidly than previous historians have supposed" (p. xiii).

This reappraisal is based on a study of close to fifteen thousand wills, supplemented by a variety of additional sources. The evidence points to a flourishing array of grammar, reading, and song schools, with increasing lay involvement both as benefactors and as clientele. Moran finds evidence of educational activity in many areas previously assumed to be dark corners of the land. She inclines, however, to a generous and optimistic interpretation, seeing schools where others might simply see acts of piety. Late medieval testators commonly provided money for children to sing or for scholars to be present at their funerals. In this study the mere mention of scholars "suggests a reading school. Evidence for children singing suggests a song school. The presence of children or scholars saying *De Profundis* offers evidence for petty learning" (p. 95). Such evidence, though plentiful, is not necessarily persuasive.

Moran is candid in her assessment of the difficulties, although her cautions are often muted by the time she draws her conclusions. In the absence of more solid material she has to rely on "indirect indicators that help the historian make an informed, or at least defensible judgment" (p. 64). She posits, speculates, assumes, and ascribes when the only alternative is silence. We watch her "following the scanty documentary leads" to reconstruct "a highly plausible chain of events" (pp. 91, 149).

The greatest difficulty comes with the treatment of literacy. Literacy is taken to mean "the ability to read a printed or written text in either Latin or the vernacular" (p. 52). This is perfectly satisfactory as a definition but virtually unusable as a standard for historical investigation. Reading, by its nature, leaves no record, so any attempt to gauge its extent must be inferential, indirect, and tentative. No assessment of literacy so measured can be compared to the signature-derived literacy rates of later centuries. It is all very well to claim, as Moran does here, that medieval literacy was "rising" and that "a significant number of laity were literate" (pp. xvi, 179). But it is simply a guess, and an optimistic one at that, to claim "a male literacy rate of between 20 and 25 percent" and an overall rate of "nearly 15 percent lay literacy" in the diocese of York on the eve of the Reformation (pp. 181, 225). These figures are derived from a curiously slapdash calculation involving population estimates and assumptions about life expectancy, numbers of schools, and school attendance figures.

Tudor and Stuart historians have claimed too much for the educational achievements of their periods; Moran is in danger of overclaiming for hers.

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THOMAS A. MASON. *Serving God and Mammon: William Juxon, 1582–1663; Bishop of London, Lord High Treasurer of England, and Archbishop of Canterbury*. Cranbury, N.J.: Associated University Presses, for University of Delaware Press, Newark. 1985. Pp. 205. $29.50.

William Juxon—bishop of London (1633–60), archbishop of Canterbury (1660–63), and lord high treasurer of England (1636–41)—is among the last of Charles I's major ministers to be the subject of a modern biography. This is no surprise. There are no Juxon papers of any substance, nor was he a particularly dramatic or controversial figure. Although he probably shared the same theological beliefs as William Laud, Juxon's personality was altogether more amiable than that of his mentor and patron. Only this can explain how someone responsible for royal financial policy during the last years of Charles I's personal rule could escape parliamentary attack when that system collapsed.

Thomas A. Mason deals effectively with the different aspects of Juxon's long career. He makes the important point that Juxon was trained as a civil lawyer, not a theologian. He shows that, even when Juxon was pursuing Arminian or Laudian policies, he did so with more tact and restraint than Laud himself or some of their other allies. Mason asserts that Arminians as such were against enclosures and against emigration to the Americas (pp. 49–53, 122), whereas it might be safer to say that Arminians usually took those views in the context of the 1630s.

The most original finding on the secular side of Juxon's career is that he acted, if not regularly at least on occasions, as a judge in the court of Star Chamber before he was a privy councillor, whereas normally only the two chief justices did so. Unfortunately, most of the references for this are cited from the author's unpublished thesis. In assessing Juxon's achievement as lord treasurer, no estimate is given of how much the total royal revenue increased during his tenure. It is certainly arguable that, if the king had kept out of foreign and domestic wars, the crown might have attained financial self-sufficiency without Parliament.

On Juxon's career during the turmoil and upheaval of the early 1640s there are two minor errors. The great council of peers did not persuade Charles I in September that the second Parliament of 1640 should be called; he announced that he was going to call it on opening that assembly (p. 133). Juxon was not formally impeached (pp. 134, 176). The document cited for this is an undated draft for his impeachment along with that of several clerical officials who had been involved in the previous prosecution of the celebrated or notorious Dr. Bastwick. An actual impeachment could not fail to have been recorded in the journals of one or both houses of Parliament.

This is an agreeably written biography based on thorough and careful research. It tells the story of a decent, quite able, but not very remarkable man who was caught up in great events in which he played a modest part but exercised little if any decisive influence.

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University of Oxford

J. KENT CLARK. *Goodwin Wharton*. New York: Oxford University Press. 1984. Pp. ix, 391. $24.95.

Goodwin Wharton, the subject of this work, led two lives. In one he was the younger son of an English peer, "alchemist, inventor, deep-sea diver, cavalry officer, MP, and Lord of the Admiralty" (p. vii). In the other he lived among marvels of the supernatural imagined by himself or suggested to him by a bizarre woman named Mary Parish. More to record his second life than his first, he wrote a memoir that J. Kent Clark uses for his biography of Wharton, one of the greatest fools who ever lived.

Wharton did not succeed as alchemist, inventor, or diver, and his military career was largely on paper; he did, however, go on the expedition to France that ended in defeat at Camaret Bay in 1694. Clark makes the most of his subject's parliamentary service and presents him as busy in Admiralty work under King William III. We also learn of his attempts to recover valuables from wrecks. For the rest, the book reflects Wharton's own greater interest in the doings of fairies, angels, and evil spirits and his claimed communication with the Christian God. Parish began it by pretending to be visited by good fairies who promised her all manner of things for Wharton, her keeper—promises that were somehow always unfulfilled. It was she who made him think two successive fairy queens were irresistibly attracted to him. Wharton wrote that later, without intermediation from Parish, he heard the Lord speak to him; he also experienced visions. Moreover, he became convinced that Mary Beatrice (James II's real queen), Queen Mary II, and Queen Anne were all so completely in love with him that only circumstances and indecision prevented his being elevated to the highest worldly station by marriage.

He had mistresses other than Parish and many would-be affairs. According to Parish, he fathered her 106 conceptions, almost all of which miscarried. Wharton believed everything she told him, despite the fact that she often made him waste time and effort attempting to find buried treasure and other fairy-tale rewards. For such a "projector," Wharton

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spent the last of his life in prosperity, though racked by bad health, still full of the wonder of his fantasies (and Parish's cheats).

Clark's failing is that he repeats statements from Wharton's memoir without distinguishing fact from imagination and with inadequate checks from other sources. (One longs for the thoroughness of Irvin Ehrenpreis's *Swift* [1962–83].) Sometimes his choice of words even suggests that he accepts Wharton's flights *au pied de la lettre*. Wharton, Clark says, often failed to record real events, and the memoir therefore adds less to the historical record than it might, while his inaccuracy—shown, for example, in recounting his experience at Camaret Bay—forbids easy reliance on him. Even the professional historian should use this book cautiously, and some will prefer not to use it at all.

GEORGE HILTON JONES
Eastern Illinois University

IAN R. CHRISTIE. *Stress and Stability in Late Eighteenth-Century Britain: Reflections on the British Avoidance of Revolution.* (Ford Lectures, 1983–84.) New York: Clarendon Press of Oxford University Press. 1984. Pp. 226. $34.50.

Common sense in historical literature is not too abundant, nor is it excessively rare. Brilliance, however, is rare, and in its departure from orthodoxy its revisionist impulse often violates common sense. The pearl beyond price is that fine balance of brilliance and common sense that subjects old and familiar dogmas to the test of new questions and assumptions. The central question of Ian R. Christie's 1984 Ford Lectures is how to account for Britain's escape from revolution in the 1790s when wild revolutionary contagion swept France, Ireland, and much of the rest of Europe. The results of this splendid work are so obvious and right that it may be justly wondered why their statement was so long in coming.

Factors of stress existed in considerable force in late Georgian Britain. The momentum of flux and change increased rapidly from the 1750s: external pressures from international conflicts, including, of course, the first encounter with "revolutionary contagion" in the thirteen North American colonies; internal tensions arising from the rapid growth in population; the increase of urban dwellers proportionate to workers on the soil; economic and social changes produced by the Industrial Revolution; unrest among the lower orders; and raucous debates among the upper over constitutional issues and parliamentary reform, which merged occasionally (as with Charles James Fox) into avowed admiration for French revolutionary doctrine. Taken together, these considerations promised fire on the mountain. But there was no conflagration, because factors of stability in British society outweighed those of stress.

First, revolutions, Christie argues, are far too closely linked to indigenous conditions to be accepted as parts of a transnational wave. Further, unlike France with its excessively privileged caste or class, Britain's social structure was infinitely graded and held together by a multitude of shared interests and concerns. A common hope of betterment in material circumstances, friendly societies and trade clubs, and religious affiliations (especially strong among Methodists and Nonconformists) all gave the individual a sense of place, status, role, security, and interest in maintaining the customary. Even the very poor had the safety net of the poor laws. Intellectual and religious thought reinforced defenders of the existing order, and, as revolutionary France became increasingly violent and atheistic, British theorists in the large majority reacted with hostility and hatred of Jacobinism. By contrast with France, Britain appeared the bastion of liberty and true enlightenment.

French revolutionary contagion created not widespread symbiotic sympathy in Britain but fear and revulsion, not the concept of the transnational brotherhood of man but the solid unity of the British people, superb and unique, and fully seized of the vision set out in time-honored Gaunt's "sceptered isle" speech in Richard II.

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RICHARD DENNIS. *English Industrial Cities of the Nineteenth Century: A Social Geography.* (Cambridge Studies in Historical Geography, number 4.) New York: Cambridge University Press. 1984. Pp. xiii, 368. $49.50.

Treading gingerly across the minefield of interdisciplinary synthesis, where the necessity to define inevitably risks extended digression, Richard Dennis selectively chooses from the methodologies of geographers, historians, and urban sociologists to produce a book that will certainly be of interest to historians. Based largely on secondary research, including monographs on selected industrial towns and cities in England and Wales, the analysis focuses mainly on the historical experience of the middle and working classes.

The Victorians' perceptions of the character of industrial cities provide the context for an empirical inquiry into the "objective" reality as indicated by quantitative evidence analyzed by researchers. Contemporaries stressed the similarity of life in Victorian cities, whereas historians have stressed its diversity. The test Dennis chooses is residential

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segregation, which he reveals to have been a gradual process, in which the middle and working classes became identifiably separate by the 1840s, which was followed by residential differentiation dividing the labor aristocracy from the rest. This process resulted from several factors: the decisions made by landowners, builders, developers, and housing managers and the growth of transport, which tended to reinforce the trend toward residential segregation. Dennis emphasizes the importance of the scale of development and that, although morphological homogeneity did not guarantee social homogeneity, a clear divide existed between dwellings intended for the lower middle class and the labor aristocracy and those rented by people with the lowest incomes.

In his attempt to locate and describe the character and dynamics of these residential enclaves, social-area analysis affords limited clues. There is surprisingly little quantitative evidence suggesting that segregation increased over time, and notions of an "artisan elite" and a "labor aristocracy" have proved an inadequate basis for identifying subtle variations within working-class areas. Even the calculation of residential persistence and mobility rates identifies only one of several factors contributing to stability, control, and "community." This is why Dennis also tries to examine employment mobility, persistence, and affiliation with and attendance of churches, working mens' clubs, and other local organizations, each of which may be at least as important in defining an individual's experience of "community" as residence.

The author uses secondary literature to support the view that the pressures of industrialization, the division of labor, differentiation in the labor force, and the sheer growth in the size of industrial cities generated segregation patterns dissimilar to those of rural mill communities: urban workers maintained an independence typically lacking in mill-town populations. Dennis concludes that, between the "decline of paternalism," which he identifies in the early Victorian period, and the rise of the welfare state in the late nineteenth century, "a new kind of working class community" was born, partly out of economic necessity and partly as leisure came to form a distinctive element in working-class lives. Without detailed studies that plot the extent and degree of overlap of the various associational networks in cities, the degree of intraclass segregation remains unknown. Dennis's research agenda, however, points the way.

ROY CHURCH
University of East Anglia

LEA CAMPOS BORALEVI. *Bentham and the Oppressed.* (European University Institute, series C, Political and Social Sciences, number 1.) New York: Walter de Gruyter. 1984. Pp. xii, 248. $40.00.

J. S. Mill set the conventional view of Jeremy Bentham, the utilitarian philosopher: he was "a boy to the last," having little experience of life, little sympathy for the full range of human emotions, and little conception of conscience, liberty, and human dignity. Lea Campos Boralevi's book attempts to depict a more complex Bentham but fails to persuade the reader that Mill was off the mark.

Boralevi considers Bentham's attitude toward "the oppressed as a whole." These include women, of whose rights he was an early, if cautious, champion. On slender evidence the author postulates a love affair (in which the utilitarian and the exiled Aaron Burr were rivals) to prove that her subject's "attitude toward women was not at all abstract" (p. 28). Oppressed also were homosexuals, about whom Bentham wrote much, and most sympathetically, favoring the decriminalization of pederasty between willing parties. He did not publish these writings, and Boralevi speculates, with as little evidence as for the love affair, that Bentham may have been homosexual. She marshals the strongest evidence to prove him more sympathetic to animals, particularly the "noble puss," than to "certain categories of oppressed human beings" (p. 166).

The poor, the natives of India, and the Jews were among the oppressed "categories" Bentham found unsympathetic. Boralevi sees the National Charity Company as consistent with the principle of the greatest happiness and finds fault with the "external point of view" of scholars who complain of the utilitarian's failure to respect the liberty and dignity of the poor. He was not more sympathetic to the Indian natives, although here too Boralevi defends his utilitarian consistency. Bentham was personally unfriendly to Jews, which causes the author to look with "greater admiration" on his defense of toleration for them.

Boralevi sees inconsistency in Bentham's position on the issue of slavery. Although opposed to the institution, the philosopher favored gradual abolition, because he believed that immediate abolition might endanger the security of society. For Bentham this meant preeminently security for property, without which he (like Hobbes and Locke before him) believed there could be no real happiness. Boralevi sees this as "a step backward," a departure from a scientific view of society to the camp of his enemies, the natural rights theorists.

Despite her previous insistence on Bentham's utilitarian consistency, Boralevi concludes that he did on occasion admit into the shaping of his opinions philosophies of natural right and of sympathy. She even sheds her disapproval of "external" criteria so as to discover, surprisingly, "libertarian conse-

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quences" in his views on the oppressed. And, finally, she suggests that Bentham intended the principle that "the more strictly we are watched, the better we behave" to apply not so much to the poor of prisons and workhouses as to "the ruling few."

The loose ends are thus conveniently though not convincingly tied, and Bentham emerges as a radical for all seasons. Bentham's "complexities" apparently inspire contradictions even in his commentators. Still, Boralevi has made a useful if somewhat controversial contribution to the recent scholarship on utilitarianism.

BERNARD SEMMEL
State University of New York, Stony Brook

H. W. ARTHURS. *"Without the Law": Administrative Justice and Legal Pluralism in Nineteenth-Century England.* Buffalo, N.Y.: University of Toronto Press. 1985. Pp. xvi, 312. $30.00.

The common law originated in a world in which there were many other courts administering a variety of different bodies of law and custom; hence, the name "common." In time its jurisdiction was extended, and royal law increasingly dominated other systems in two distinct ways. One was by imposing its own rules on other courts; the other was by claiming and asserting supremacy over them, for example, by the use of procedures restricting the scope of other jurisdictions. But the movement toward a single, monolithic legal system—one common law for one nation-state—has never been completed. The United Kingdom, or even a lesser legal unit such as England and Wales, remains home to legal pluralism, where personal laws can still hold sway; this is probably the case everywhere in the world.

This study of the nineteenth-century common law and its rivals aims first to demonstrate the persistence of pluralism and to criticize, for example, the attitude of mind expressed in A. V. Dicey's identification of the rule of law with the rule of Westminster Hall. It attacks the dominance of what H. W. Arthurs calls the centralist paradigm of law. The emphasis is on systems that were in some sense at odds with the common law; hence, by no means all rivals are discussed here.

Arthurs then goes on to show how, during the nineteenth century, two conflicting processes were at work. One involved the extension of the common law at the expense of other systems, such as through the extinction of the old courts of requests and the establishment of the modern county courts, as well as through attacks on traditional mercantile arbitration. The other involved the development of systems of administrative jurisdiction, such as through the work of the official inspectorates established, for example, to regulate coal mining. This latter process, which produced a new pluralism, is an aspect of the general increase in government during this period, made necessary by the inability of the common law to respond to the pressures of industrial and commercial expansion. For, to an extent probably underestimated, the common law system of litigation was largely irrelevant to the mass of the working population of Victorian Britain; it did nothing for them.

This book, based on much original work (such as on the courts of requests), does not, and cannot because of its scope, offer a definitive study of such specialized topics as commercial arbitration during this period. The book aims instead at reorientating historical inquiry into Victorian law. So far as the common law is concerned, it redefines the context in which the system existed. In a sense legal historians have always known about legal pluralism; this study serves, however, to spell out more sharply the significance of that knowledge. Arthurs's treatment of his subject reflects, but is not dominated by, ideas associated with the critical legal studies movement, and, although primarily a valuable contribution to legal history, it also has important things to say for legal theory.

A. W. BRIAN SIMPSON
University of Chicago

ROBERT BUD and GERRYLYNN K. ROBERTS. *Science versus Practice: Chemistry in Victorian Britain.* Dover, N.H.: Manchester University Press. 1984. Pp. 236. $35.00.

In the late 1960s and in the 1970s, both English and American policy makers debated the nature of the relationship between science and technology. Did funding of fundamental research produce an automatic payoff by engendering technical innovation? In another form the same question occupied historians of science and historians of technology. Some historians of technology argued that technological know-how was a necessary precursor to scientific discovery: humankind had exploited the wheel long before pi was understood, and thermodynamics owed more to the steam engine than the steam engine owed to thermodynamics. Some historians of science argued the other way, although their expositions generally were not bolstered by such dramatic and easily grasped examples. A few scholars balanced the claims of both sides, arguing that whatever relationship prevailed depended on the time, place, and subject under discussion. Intrinsic to the debate were questions of definition: what, precisely, did the terms "pure" and "applied" science mean? As the debate continued, less common

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rubrics made their appearance, including some like "applied technology" and "pure engineering."

Robert Bud and Gerrylynn K. Roberts investigate this problem, using as their focal point the development of British chemistry, "one of the most 'practical' of sciences in one of the most pragmatic of nations" (p. 11). They chart the course of the field from the early 1800s to the end of the nineteenth century, emphasizing the period 1830–70. In those forty years, the authors conclude, chemistry became increasingly defined in academic terms even as chemical industries bloomed; moreover, a consensus arose as to what was "pure" science and what was "applied." Bud and Roberts's choice of time, subject, and place is ideal.

At the beginning of the nineteenth century, Scottish sentiments about the coincidence of interests between the manufacturing community and the community of natural philosophers dominated. Even though no well-defined academic group was specifically labeled "chemists," many practitioners earned their living by doing chemistry, and they were scarcely "amateurs." The chemistry section of the fledgling British Association for the Advancement of Science cheerfully reported both industrial and theoretical advances. As the century proceeded, however, academics trained either in Scotland or in Germany (and, occasionally, in both places) helped found the Chemical Society (1841) and the Royal College of Chemistry (1845). Chemical propagandists promoted chemistry for its ability to provide solutions to practical problems; the academic community heard that message, and the number of academic chemists swelled appropriately. Nonetheless, practical matters surfaced only at the end of syllabi, and most students took just the beginning of courses. The rationale for these courses posited knowledge defined as "scientific" as a necessary antecedent to knowledge defined as "practical." Prosopographical studies of the members of the Chemical Society, of the alumni of the Royal College of Chemistry, and of "chemical" patentees shows how complicated the relationships were between the groups and how ill defined the areas. Chapter 5 demonstrates how "pure" chemistry separated from "applied" by focusing on science education generally. In the final thirty years of the century academics increasingly dominated the chemical community, so that by 1900 to become a British chemist required academic training in theoretical matters, with only a nod to more practical concerns.

Concentrating not on substantive chemical discoveries but on the philosophies and the activities of the British chemical community, Bud and Roberts's splendid historical study illuminates the complexities in the relationships between "pure" and "applied" science. Their demonstration of how knowledge of chemistry became part of British liberal education is a scholarly bonus.

JUNE Z. FULLMER
Ohio State University

MARTHA S. VOGELER. *Frederic Harrison: The Vocations of a Positivist*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xvii, 493. $49.95.

Frederic Harrison was the best known of the British followers of Auguste Comte. A prolific man of letters, he contributed scores of articles to Victorian periodicals on subjects as diverse as religion, trade union rights and reform, contemporary politics, and literary criticism. He was a person who had the uncanny ability to be at the right place at the right time. For example, he made his first real splash in public life with an article that criticized the liberal Anglican contributors to *Essays and Reviews* for being too moderate when virtually all other commentators regarded them as overly radical. In many such instances he proved able to ride the coattails of a current dispute without every really instigating one himself and without making particularly original contributions to the intellectual activity of his day.

Harrison was born into a comfortable life that afforded him an Oxford education and the possibility of indifference to his law practice after his time at the Inns of Court. His family's wealth, which he supplemented later by writing, also permitted him to work through a double loss of faith. The first was a loss of faith in the more or less traditional Christianity of his childhood. The second was the abandonment of mainline middle-class Victorian politics for a radicalism sympathetic to the working class. In place of Christianity he adopted Comte's religion of humanity and became one of its two chief British advocates, the other being Richard Congreve. In the political arena he worked on a parliamentary commission to improve the rights of unions, and in newspapers and journals he campaigned for the welfare of the working class.

Nonetheless, when all is said and done, Frederic Harrison looked in the eye one great issue of his day after another and always blinked. Where others pursued either liberal Christianity or agnosticism, Harrison followed the improbable course of Comte's cult. He preached the ideal of a wider humanity for much of his life, but after the turn of the century he became a Germanophobe. He honestly supported the advance of trade unionism but enjoyed weekends at the estate of Andrew Carnegie. Martha S. Vogeler treats all of these inner tensions and inconsistencies with sympathy, but another biographer might have approached them with the satire of a Tom Wolfe commenting on the radical chic of the Victorian era.

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This well-researched book is the first biography of this remarkable, if deeply flawed, Victorian, and Vogeler is very much alive to the cultural crosscurrents of late Victorian and Edwardian Britain. Because Harrison interacted with so wide a variety of his contemporaries, scholars in English, labor, and intellectual history will find the biography valuable. My one real criticism is Vogeler's idea that positivism provided a vocation for Harrison. Her pages seem, on the contrary, to demonstrate that positivism was the road of a man who never really discovered his vocation.

FRANK M. TURNER
Yale University

PETER STANSKY. *Redesigning the World: William Morris, the 1880s, and the Arts and Crafts*. Princeton: Princeton University Press. 1985. Pp. xvi, 293. $27.50.

Echoing the title of a talk given by Walter Crane in 1888 under the auspices of the Arts and Crafts Exhibition Society, Peter Stansky explains at the close of this book that his aim has been to discuss "The Arts and Crafts Movement: Its Several Tendencies and Possible Outcome" (p. 260). This is a misleadingly simple evaluation of the volume. Throughout the study Stansky traces two themes, both fraught with ambivalence and paradox, and the result is a rich illustration of political and cultural history intertwined.

The first theme concerns the ways in which the English Arts and Crafts movement, under the dominant influence of William Morris, led design away from Victorianism to modernism. The designers who followed the teachings and practice of Morris departed from excessive decoration and slavish imitation of dead historical styles and sought to achieve a simple "vernacular" style in which practicality was not the least aspect of an object's beauty. Whatever the object produced, whether a book or a house, the artists and craftsmen concerned themselves with the totality of the design, in which all art forms contributed equally to produce a harmonious, rational whole.

This was the direction of modernism, more appreciated on the Continent than in England. Yet the Arts and Crafts movement had roots in the English past, in the Gothic revival and the theories of Augustus Pugin and John Ruskin. Morris and many of his disciples were markedly unsympathetic to avant-garde styles, condemning art nouveau, for example, as an "insidious disease" (pp. 169–70). What complicates the issue even further, as Stansky explains, is the presence of art nouveau elements in the work of many of those same men who lambasted it.

The second theme, as central to the book as the first, is the parallel between revolutionary activity in the world of design and ferment in the political sphere. Stansky reiterates that the same dissatisfactions with society, the same aspirations toward freedom from restrictive established authorities, motivated the men who formed craft guilds and those who launched socialist societies in the 1880s. The intricate connection between Morris's political and artistic activities has been analyzed many times before, as have the inconsistencies of a Marxist theorist whose flourishing capitalist enterprise produced beautiful home decorations generally too expensive for the common people to buy. Stansky's contribution is to move beyond Morris and trace many of the same tensions in the work of his disciples, particularly as exemplified in three important Arts and Crafts organizations established in the 1880s: the Century Guild, the Art Workers' Guild, and the Arts and Crafts Exhibition Society. Although by no means all the members of these associations shared Morris's political convictions, many recognized with him the incongruity of gentlemen amateurs trying to reform the conditions of labor in a bourgeois society. What, after all, could be more bourgeois than an organization (like the Art Workers' Guild) whose minutes record the members' anxiety over lost umbrellas (p. 156)?

The quality of the book itself—materials, cover, design—is worthy of the subject. Princeton University Press should be congratulated.

JANET OPPENHEIM
American University

JOHN BENSON, editor. *The Working Class in England, 1875–1914*. Dover, N.H.: Croom Helm. 1985. Pp. 214. $27.50.

In his brief introduction, John Benson promises to give his readers six "up to date, wide ranging and provocative studies on aspects of later Victorian and Edwardian working class life." A worthy goal, but to what extent have he and the other contributors succeeded?

The two essays that deal most explicitly with traditional labor history topics are certainly provocative. M. J. Haynes begins his piece, "Strikes," with some skeptical remarks (not wholly unjustified) on the use of strike statistics by James Cronin and others. He next discusses the inadequacy of the social, economic, and organizational factors that have commonly been advanced as explanations for the incidence of strikes. Finally, he suggests that historians of strikes should give more serious attention to those changes in workers' consciousness that Tom Mann labeled "psychic waves." His contribution thus becomes an interesting, if inconclusive,

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excursion into the history of popular mentalities. Benson's related essay, "Work," challenges the view that a major trend of the period was the growing homogeneity of workers (resulting from narrowing distinctions between skilled and unskilled, the convergence of regional wage rates, and the like). While not denying the existence of many of these changes, Benson convincingly argues that heterogeneity—and therefore continuity—were far more important. Regionalism remained strong; both petty entrepreneurialism and petty crime were pervasive (if rather incongruous) features of workers' communities; and even by 1914 over three-fourths of the working population remained outside the control of labor unions.

Three of the essays bear close relationships to recent books by their respective authors. F. B. Smith's contribution on health gives him an opportunity to refine some of the arguments he made in *The People's Health, 1830–1910* (1979) and to add new material, especially on the Celtic fringe. Elizabeth Roberts looks at the family almost exclusively in the context of the three Lancashire towns (Lancaster, Barrow, and Preston) that provided the material for her book *A Woman's Place: An Oral History of Working-Class Women, 1890–1940* (1984). Despite this narrow focus, her use of numerous vivid quotations from 170 elderly informants makes hers the most entertaining essay in the collection. Hugh Cunningham's piece on leisure forms a pendant to his book, *Leisure in the Industrial Revolution, c. 1780–c. 1880* (1980), in which he argues that the stage was set in the third quarter of the nineteenth century for "the full emergence of working-class demand for leisure" (p. 183). In this essay he shows the numerous ways in which "the structuring of leisure acquired the force of custom" from 1875 to 1914 (p. 157). By discussing aspects of supply and demand and specifying the role of leisure in the life cycle, yearly cycle, and weekly cycle of workers, he breaks more new theoretical ground than any of the other contributors.

The collection ends with David Woods's account, "Community Violence." Supplementing official statistics with various unofficial accounts of common assaults, assaults on police, and aggravated assaults on women and children, Woods demonstrates that the incidence of community violence declined markedly after about 1880. His special interest in one region (the Black Country) enables him to uncover some colorful examples, like the irrepressible Bridget Murphy, "Queen of Hell Fold," Walsall.

All in all, the editor's promises have indeed been fulfilled. Although some readers may wish for a larger number of essays on a wider range of topics, this is a stimulating collection nonetheless. I can recommend it highly to all specialists in Victorian and Edwardian social history.

ROBERT GLEN
University of New Haven

GILLIAN SUTHERLAND. *Ability, Merit, and Measurement: Mental Testing and English Education, 1880–1914*. New York: Clarendon Press of Oxford University Press. 1984. Pp. ix, 332. $45.00.

Gillian Sutherland set out to "investigate the roles played by the idea and techniques of measurement in the English public education system" (p. 1), and she has produced an ambitious work that makes an important contribution to statistical, educational, and social history.

The investigation is divided into two parts. The first three chapters examine the development of testing handicapped children. Seeing the 1880s as a watershed, Sutherland explains the convergence of compulsory attendance, "payment by result," and growing sensitivity to the child who might not be educable; the result was acceptance of the idea of special schools by 1914. The process of classification at work here arose from a complex of nineteenth-century social attitudes and policies. By the 1890s these ideas assumed a scientific aspect as theories of eugenics and as tests of intelligence.

The idea of measurement, which first developed in treating disabled children, was soon extended to group testing of "general intelligence," with some of the major advances in that direction coming from France and the United States. By the interwar period, as Sutherland explains in her second major section, this testing was being debated and applied, less widely than some think but on a broad enough basis to be a major national educational issue. Testing at age "eleven plus" to determine the advancement from primary to secondary level emerged in the 1920s and 1930s. Sutherland estimates that three-fourths of the Local Education Authorities in England did this type of testing by 1940, at one time or another. She also shows the great regional variation in testing patterns. Absence of a standard and the frequent use of "homemade" tests of dubious value complicate an already confusing scene. Sutherland makes a valiant effort to sort out the diversity with maps and tables, but the sorting cannot alter the basic situation.

As Sutherland points out when criticizing authors who have seen "dominance of mental measurement" in the 1930s, there was too much variety, too much local influence, and too much English elitism to allow testing to dominate anything. Her unhappy

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conclusion is that the science of testing was not as influential as the sophistry of the establishment.

RICHARD S. TOMPSON
University of Utah

FRANCIS WEST. *Gilbert Murray: A Life*. New York: St. Martin's or Croom Helm, London. 1984. Pp. vi, 265. $25.00.

Gilbert Murray was the most brilliant Oxford undergraduate of his generation, winning the major classical prizes—the Hertford and the Ireland—in his first year. His career was an unbroken series of personal triumphs: he was prize fellow of New College and then professor of Greek at Glasgow at age twenty-three; he married into the center of the Whig intellectual aristocracy; recognized as the foremost translator and popularizer of Greek drama for his generation, he was Regius Professor of Greek at Oxford for thirty-six years; a prominent Liberal politician, he chaired the League of Nations Union from 1923 to 1937 and received the Order of Merit; and he was buried in the Poets' Corner of Westminster Abbey. Yet the portrait that emerges from Francis West's biography is by no means a happy one. Murray saw himself more as a poet and dramatist than as a pure classical scholar, but his plays were unsuccessful and his translations plummeted in popularity after World War I. His scholarship was never considered exact enough by the fastidious, his edition of Euripides was never completed, and his edition of Aeschylus was so flawed that it had to be completely redone. As a politician, Murray lived to see the destruction of the Victorian liberalism for which he stood. In his family life Murray's relationship with his wife was often difficult, and all his children violently rejected in one way or another his ideals and his example.

The major theme of West's biography is the portrayal of Murray's steadfast adherence to rationalistic, humanitarian, and anticlerical values already fully formed in youth. West demonstrates convincingly how these values permeated both Murray's scholarship and his politics. West also shows considerable tact and judgment in his discussion of some of the more controversial aspects of Murray's life and career, especially his rejection of pacifism in 1914 and the genuineness of his supposed deathbed reception into the Roman Catholic Church.

This reader was often inconvenienced, however, by West's decision to omit reference notes. Their absence often made it impossible to form an independent judgment on such questions as the degree to which West relied on Murray's unfinished autobiography. Also, for the historian of academic life, notes might have either eliminated several questions or, at the least, pointed the way toward answers: Who were the other candidates for the Greek chair in Glasgow? And was West perhaps too quick in attributing to political bias opposition to the appointment of so young a candidate? How was it that Murray's tutorial fellowship at New College entailed only two hours a week of teaching when the general impression of the college tutor circa 1905 is of an overburdened "teaching drudge"? Why was Murray asked to continue in his Oxford chair beyond the normal retirement age when there had been complaints previously from the vice-chancellor of his overinvolvement in political work and his lack of devotion to academic duties?

Perhaps there is also a larger artistic flaw in this biography: although West clearly admires Murray despite recognizing his flaws, this reader came away with a much more marked impression of Murray's private limitations (priggishness, self-satisfaction, and personal insensitivity) than of his public virtues (high political ideals and effectiveness in communicating the "relevance" of Greek drama to modern society).

ARTHUR ENGEL
Virginia Commonwealth University

JAMES PATRICK. *The Magdalen Metaphysicals: Idealism and Orthodoxy at Oxford, 1901–1945*. Macon, Ga.: Mercer University Press. 1985. Pp. xliii, 190.

Who were the "Magdalen metaphysicals"? James Patrick identifies them as Clement Webb, John Alexander Smith, R. G. Collingwood, and C. S. Lewis, all of whom were fellows of Magdalen College in the early twentieth century. They were united by opposition to philosophical realism, the dominant school of philosophy at Oxford, and shared certain common views about religion, philosophy, and history.

In his first chapter Patrick describes the position of idealism at Oxford during the Edwardian period, beginning with the enormous influence of T. H. Green's idealism and explaining its decline. What is exemplary in Patrick's account is the attention paid to the weaknesses within the idealist tradition, particularly its relation to theology and the natural sciences. His conclusion is that, although idealism still "held the ground by habit and default" (p. 20), it was being challenged by such philosophers as John Cook Wilson, H. A. Prichard, and H. W. B. Joseph. By 1918 the realism of Cook Wilson and his school had become the "fashionable philosophy" at Oxford (p. xi).

Against this background Patrick examines the ideas of his four protagonists. Webb was at once historian and philosopher: his publications include studies in medieval intellectual history as well as works dealing with the relationship between theol-

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ogy and philosophy. As against the realists, Webb maintained that the metaphysical questions put forward by philosophers from Plato to Kant remained pertinent and that "real" knowledge resulted from the activities of the thinking mind. Patrick defines Webb's theological position as that of a "believing rationalist" (p. 43). Smith, who became Waynflete Professor of Moral and Metaphysical Philosophy in 1910, shared Webb's interest in history, and especially in the philosophy of history. Croce's thought was introduced to Oxford through Smith, who continued to espouse Crocean idealism until his retirement in 1935. Smith wrote little, but through teaching, conversation, and talent for friendship he made his presence felt at Oxford. Among the younger generation he influenced both Collingwood and Lewis, and in 1935 Collingwood became Smith's successor in the Waynflete professorship.

The chapters devoted to Collingwood and Lewis command special interest because of the stature and renown of these men. Patrick is at his best in unraveling the complexities of Collingwood's philosophy and, particularly, in showing the centrality of Collingwood's religious beliefs to the various aspects of his thought. Historians will be interested in an exposition that places Collingwood's philosophy of history firmly within the context of an idealism that owed something to Croce but far more to Collingwood's own reworking of the history of Western philosophy.

After these rigors of exegesis the reader will turn with pleasure to Patrick's analysis of the far more accessible writings of Lewis. Two themes dominate the chapter on Lewis. There is the account of his move from realism through idealism to pantheism, followed finally in 1931 by his conversion to Christianity. The other theme centers on Lewis's brilliant Christian apologetic, of which Patrick takes *Pilgrim's Regress* (1933) to be the central text.

One of the strengths of this book is the author's subtle and judicious treatment of the question of influence, both among the "Magdalen metaphysicals" and on others. He does not indulge in exaggerated claims: in the Oxford of the 1930s the "Magdalen metaphysicals" and their adherents were an isolated if combative minority. The triumphant school was that of analytical philosophy, which developed out of the earlier realism.

It should be noted that Patrick makes no pretense to impartiality, but his own intellectual and religious commitments have enabled him to write with knowledge and understanding about a hitherto-neglected school of thought. His book is a welcome addition to the literature on British intellectual history, not least because it presents the position of those who were the losers—temporarily, Patrick would argue—in one of the significant contests of twentieth-century philosophy.

DORIS S. GOLDSTEIN
Yeshiva University

EDWARD SMITHIES. *The Black Economy in England since 1914*. New York: Gill and Macmillan or Humanities, Atlantic Highlands, N.J. 1984. Pp. 165. $38.00.

The term "black economy" received wide currency in Britain during the 1970s to describe the informal and, above all, untaxed sections of the economy. In this short book Edward Smithies attempts to trace the history of the "black economy" from the First World War to the present. He defines his ground as "principally the history of black markets, tax fraud, smuggling and pilfering" (p. 5), taking evidence from five areas in particular: Birkenhead, Barnsley, Walsall, Brighton and Hove, and parts of East London. Within a firmly chronological framework, Smithies gives a useful account of some of the less well known aspects of economic activity in twentieth-century Britain—aspects that often reveal a far less conformist and also more enterprising population than is sometimes assumed. He offers some informative sidelights on how people managed during the depression as well as a careful appraisal of the role of the "black market" in the Second World War. Unlike some occupied countries of Europe where a national "black market" came into existence, Britain's tightly controlled administrative structure for the rationing and distribution of goods was never seriously challenged, although thousands of "local" black market transactions took place. Smithies, too, offers some interesting items on the various "fiddles" being practiced in the "austerity" years of the Attlee governments. He also gives an account of the rise of the "tax avoidance" industry in Britain through which businesses and individuals seek to minimize taxation or avoid it altogether.

This last point, however, is symptomatic of a problem with the book as a whole. "Tax avoidance" is not "tax evasion"; in Britain the distinction denotes legal as opposed to illegal forms of reducing tax paid. The former is only "tax fraud" within a much looser definition of the "black economy" than that offered by Smithies. To confuse matters further, at one point he speaks simply of "tax evasion by the rich" (p. 134). In fact, the last sections of this book suggest a rather weak conceptual framework overall. This is not a thoroughgoing study of what might be called the "informal economy" and its place in the economy and culture of different classes, although a comparison of attitudes of different classes to taxation would be most interesting. Nor is it a serious study of the extent of what most people recognize as the "black economy"—the pay-

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ing for services and goods in cash, which allows the payee to avoid taxation and the payer to obtain a discount or special treatment. The impact of this book for serious scholars is therefore far slighter than it might have been.

JOHN STEVENSON
University of Sheffield

RITCHIE OVENDALE, editor. *The Foreign Policy of the British Labour Governments, 1945–1951*. Leicester, England: Leicester University Press; distributed by Merrimack Publishers' Circle, Salem, N.H. 1984. Pp. x, 198. $24.00.

More than many such collections, this volume presents a coherent and generally convincing assessment of the subject listed on its title page. The product of a conference held at the University College of Wales, Aberystwyth, in 1982, the essays explore the ways in which the Labour government of 1945–51, to use the words of Ritchie Ovendale, "preserved Britain's great power status at the conference table, secured the Anglo-American special relationship on which Britain's post-war foreign policy has continued to be based, and presided over the change from British Commonwealth to Commonwealth and the moves towards independence of over a quarter of the world's population" (pp. 17–18).

As the editor concludes, there is no doubt that in terms of what those governments set out to do the result was a considerable achievement. But the individual essays raise questions that make it necessary to qualify the achievement in a variety of ways. Although Ovendale judges in his paper on the cold war in Asia that the astute diplomacy of Ernest Bevin and other officials ensured the substantial unanimity of American and British policies, he nevertheless reveals in his materials the significant limits of British influence on America's role in Asia. In a somewhat different fashion, the late F. S. Northedge reminds us that British policy in the Middle East was a dramatic failure, in part because of British inability to understand the changes, brought about by the Second World War, in the thinking of non-European peoples about the possibilities open to them.

Even more critical is D. K. Fieldhouse, whose essay probes the impulses and forces that influenced the Labour governments' policies on imperial and Commonwealth matters. He argues that, whatever the domestic crises that appeared to make it necessary, Britain between 1945 and 1951 exploited, more severely than at any time since overseas colonies were established, those dependencies that were unable to defend their own interests. Dismissing the notion of a systematic, planned program of decolonization, he concludes that the vision of a new Commonwealth to serve as a "machine for supporting Britain's role as a great power" was a chimera, however much Labour clung to it as a possible alternative to close alignment with Europe or even America.

After an introduction that too sweepingly brands most American writing on the origins of the cold war as "unhistorical, if not anti-historical" in character (p. 45), D. Cameron Watt carefully details the steps by which the American and especially the British governments came to their deep belief in the fundamental hostility of the Soviet government that characterized the cold war. Geoffrey Warner then surveys the Labour government's policy toward Western Europe, taking issue with the view that it was a catalogue of missed opportunities. Indeed, contemplating the "wine lakes and butter mountains" of the European Economic Community, he wonders whether Bevin, rather than Jean Monnet, did not have a firmer grasp of the line between idealism and practical politics.

Prefaced by a warm and appreciative appraisal of Bevin by Sir Frank Roberts, who served as his principal private secretary from 1947 to 1949, this little volume can usefully take its place alongside such studies as Alan Bullock's *Ernest Bevin, Foreign Secretary* (1983) and William Roger Louis's *The British Empire in the Middle East, 1945–1951* (1984) as a major contribution to our understanding of Britain's international policy immediately after the Second World War.

HENRY R. WINKLER
University of Cincinnati

PAUL A. WELSBY. *A History of the Church of England, 1945–1980*. New York: Oxford University Press. 1984. Pp. xii, 300. $29.95.

Because the Church of England remains an established church, its history remains peculiarly important for understanding English life and culture. Paul A. Welsby proves himself an expert guide to the developments in the Church of England from 1945 to 1980, showing us both its intricate internal history and also the ways in which church and state nudged and worried one another. He divides his account into three broad sections, each roughly coinciding with the primacy of a different archbishop of Canterbury: Geoffrey Fisher (1945–61), Michael Ramsey (1961–75), and Donald Coggan (1975–80). The book contains just the right amount of detail—the story is fully told, but one is never bogged down in the minutiae—and the detail Welsby provides is unerringly accurate.

The author tells us in his preface that the "writing of contemporary history can be a hazardous enterprise," and his book illustrates the point. The very structure of the volume, in which ten-year periods

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override topics and themes, warns us that no evaluation or analysis will be found and that the treatment will be defined by committees, reports, and important people. In this sense, it is an old-fashioned history.

The chief omission from Welsby's book is an understanding of the relentless decline of the Church of England, both as an institution and as a focus of English religious aspiration, during the period under study. Attempts to reorganize and rationalize the church's government, ministry, and parochial structure have all come too late and have failed to grasp the problems, with the result that it is now considerably weaker than it was forty years ago. In matters of faith and worship, Anglican religion has fared no better; only the evangelical and conservative strand has held its ground, whereas radicals and Anglo-Catholics have fallen back and the center has virtually collapsed. The people of England are not less religious than they were forty years ago, but progressively fewer of them find it possible to express their religiosity within the Church of England, and the majority view the church itself as a religious failure. Controversialists such as John Robinson have evoked a profound reaction from ordinary people, but, although they sense the importance of the issues, churchgoing as a response has lost credibility.

Perhaps it is unreasonable to expect an Anglican parson to draw these conclusions and to illustrate them from recent events. At the end of the day it does not matter, because Welsby has provided us with a marvellous blow-by-blow account of the events of which history is made. Those already familiar with the recent past will find his book an invaluable record; those outside the Church of England will find in the book a readable and reliable guide.

ROBERT TOWLER
Independent Broadcasting Authority
London, England

JERROLD I. CASWAY. *Owen Roe O'Neill and the Struggle for Catholic Ireland.* Philadelphia: University of Pennsylvania Press. 1984. Pp. xiii, 353. $40.00.

In the aftermath of the signal victory over the Scottish covenanter army at Benburb in 1646, Pope Innocent X presented General Owen Roe O'Neill with the handsome sword of his uncle, Hugh O'Neill, earl of Tyrone. This gesture merely intensified the suspicions with which the Ulster general's aspirations were viewed by his nominal allies in Ireland, the Catholic confederates and the royalists. Unlike his relative, who died in exile in Rome in 1616, Owen Roe spent his final years engaged in desperate military and diplomatic action to achieve his religious and political ends, which encompassed the restoration of his own family estates. Whereas Earl Hugh consistently has received much attention from modern scholars, the career of the younger O'Neill has hitherto been underresearched if respectfully noted, perhaps because of the very complexity of the issues that dominated his later life.

Jerrold I. Casway's superb account makes ample amends for this comparative neglect. His subject's long period of exile, which shaped his determined ambitions, was caused by the failure of Hugh O'Neill's struggle in association with the political Counter Reformation in Europe to resist England's assertion of power in his native Ulster. Owen Roe's growing military prowess in the Spanish-sponsored Irish regiment in Flanders was marked by ominous political and cultural divisions between the Old Irish and Old English groups among the exile community. Owen's decision to return to Ireland in 1642, after years of disillusionment with Spain's vacillating policies, was based on his urgent hope that the best chance to fulfill his aims was accommodation with the beleaguered English king's Irish party in concert with the Anglo-Irish Catholics. Using the evidence of O'Neill's letters and other sources, Casway shows how, in spite of adverse economic and political circumstances, a disorganized rabble was molded by iron discipline into a fighting force capable of winning the field at Benburb. As the reverberations of that victory died away, however, the fragile unity of Irish Catholics—which was strained by personal animosities, ethnic prejudice, and the ill-advised intervention of Giovanni Rinuccini, the papal nuncio—finally broke down, and the full agreement on religion and status, which the dying Owen Roe negotiated with the royal representative, was rendered irrelevant by the indiscriminant conquests of Cromwell's army.

Casway's main achievement is to draw a coherent and penetrating portrait from the widely scattered sources on O'Neill's rootless life. Objective yet compassionate, this study compels admiration and sympathy for the subject, even if at times the motives and actions of the members of the Anglo-Irish Catholics of the Confederation of Kilkenny, Owen Roe's great rivals, seem to suffer unduly through unfavorable comparison with those of the central figure. Casway's biography is a distinguished contribution to the study of early modern Irish history.

COLM LENNON
St. Patrick's College

KEVIN O'NEILL. *Family and Farm in Pre-Famine Ireland: The Parish of Killashandra.* Madison: University of Wisconsin Press. 1984. Pp. xiv, 232. $25.00.

The 1841 Irish census is one of the most remarkable statistical documents of the nineteenth century. In comparison with similar compilations it is unusually

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rich in detail, accurate, encompassing, and consistent. Although some scholars, especially P. M. A. Bourke and J. J. Lee, have criticized aspects of it, most scholars agree that the census is an indispensable source resulting from an unusual administrative effort. The published data are largely summaries on the county level; most of the enumerator forms were destroyed in the fire at Four Courts, Dublin, in 1922. Some fragments have survived, and the biggest is for the entire parish of Killashandra, County Cavan in south Ulster.

These returns form the core of the "Killashandra study file" used by Kevin O'Neill to reconstruct the economic and social characteristics of this community. The file consists of over twelve thousand individuals in over twenty-two hundred households. The census provides data on the family's size and history and the ages, literacy, and occupations of its members. The original forms (called "forms B"), which included data on farms and housing quality, were not preserved. O'Neill complements his census manuscripts with tithe applotment books, valuations, parish records, and government papers such as the Poor Law Inquiry (1836) and the Devon Commission (1845).

Using this statistical foundation, O'Neill provides much interesting information and comes to some sensible conclusions. He points to the shortage of capital as a major factor in the backwardness and poverty of the region, distinguishing correctly between the overall scarcity and the misallocation of capital. He also rejects the traditional, simplistic description of Ireland as a classic case of Malthusian overpopulation in favor of a more sophisticated model of a heterogeneous peasant economy.

In spite of the richness of the sources and the importance of the topic, this book turns out to be a disappointment. To start with, it seems as if O'Neill completed his manuscript in 1978 and did not deem it necessary to update it prior to publication. As a result, he ignores all work published on the subject in the past six years. He also makes no attempt to compare the Cavan experience with that of other counties (for example, County Mayo, recently analyzed by Eric Almquist). The book contains some serious misstatements and *ex cathedra* assertions without support. For instance, we are told that by 1841 the landless were the majority in Cavan's rural society (p. 101). This is simply incorrect: in 1841 there were 41,000 families in rural Cavan, of which 31,500 were chiefly employed in agriculture. How many farms were there? The census reported 25,500 but actually understated the number of farms. The poor law data (not used by him) report a total of 28,300 farms. Either way, the landless are shown to be a minority. O'Neill also concludes on the basis of the tithe applotment books that the median (and mean) farm size was approximately sixteen acres in 1841 (p. 43). The median is, of course, much less than the mean because the distribution was heavily skewed. The poor law union data for the Union of Cavan (of which Killashandra comprised about 13 percent) give a median farm size of about ten acres and a mean size of thirteen acres (assuming that the proportion of land under cultivation for the union was the same as for the county). No serious evidence is produced to back up *obiter dicta* like "women and children [of laborers] seldom had the luxury of footwear" (p. 117). And he apparently does not understand the *modus operandi* of the competitive markets for land and labor he describes since he writes that "nothing could indicate more clearly the lack of employment opportunity for Cavan laborers than this [the conacre] system in which laborers literally had to bid against one another for the opportunity of working" (p. 105).

The deepest flaw of this book is, however, the artificial distinction between farmers and laborers and the search for patterns within these two groups. The fact is that most rural families in Cavan (and elsewhere in Ireland) had access to some land. Some of these farms were so small that the family had an excess supply of labor while other farmers needed to hire additional labor. The farm would thus either demand or supply extrafamilial labor depending on family size, the opportunities for cottage industry, the quantity and quality of land, and so on. Thus, not all "farmers" were "laborers" but most "laborers" were also "farmers." In that context notions such as "unemployment" can be quite hazardous, and it cannot be said that O'Neill's discussion of underemployment is very enlightening. The chapter on population, which is completely based on the juxtaposition of laborers and farmers, is therefore also flawed. Quite possibly, the distinction between the two groups only conceals richer and more interesting patterns.

In sum, given the wealth of the data he has collected, O'Neill has missed an opportunity to create a classic quantitative study of a peasant society. A more rigorous and precise analysis of these households could be made, and much more about Killashandra could be learned from these statistics if only someone asked the right questions.

JOEL MOKYR
Northwestern University

K. THEODORE HOPPEN. *Elections, Politics, and Society in Ireland, 1832–1885*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xix, 569. $49.95.

This time one of them has really done it—"them" being the enterprising, mostly young, scholars who have turned the fabric of nineteenth-century Irish society and politics inside out during the last decade.

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What K. Theodore Hoppen has done so well is examine the social, economic, and cultural anatomy of Ireland as it relates, even remotely, to the political process. And, instead of tying his findings retrospectively to the two great periods of national consciousness—the O'Connell era of the 1830s and 1840s and the Parnell period of the 1880s—he discerns the local concerns of the decades in between as more representative of the normal course of Irish politics, extending even into the twentieth century.

Hoppen begins with an examination of the changing nature of the franchise and the electorate. The Franchise Act of 1850, although it encompassed less than 5 percent of the population, had especial impact in a society in which the influence of the artisan was being displaced by that of the shopkeeper. In the countryside the declining influence of the "big house" is viewed against the rise of the small farmer class. The chapter on the church delves into such seemingly obscure corners as the reading matter of parish priests, the education of seminarians at Maynooth, and the old world of fairies, witches, and banshees to explain the extent of the so-called devotional revolution. But the broad framework of social, economic, and cultural change "drew the contours within which the new forces were obliged to operate" (p. 256). Hoppen's dissection of political parties, where he refers to the Conservatives as "an authentic, lively, and influential tradition" and the Liberals as "an example of effective political parasitism" (p. 257), is equally masterful and complete. A somewhat lengthy chapter on violence ensues before a final turning to the nationalist mode of politics in the 1880s. To say that the latter chapter is more restrained than other portions of this strongly argued book is merely to admit that the author has maintained his emphasis on local and material considerations. "The parish pump has long been the true symbol of the 'hidden Ireland,' and its deep and constant importance is best revealed in those periods when it was able to emerge from the closet of national mobilization and nationalist rhetoric into the full light of demotic day" (p. 436).

What makes Hoppen's work so impressive is the sheer weight of evidence through which his localist interpretation is argued. The extent of his documentation is awesome, featuring not only a massive accumulation of original sources and secondary works but also an extensive body of statistical (in fifty-six tables) and literary materials. Yet the text is written with such style, imagination, and good humor that the effect is almost devastating because so unexpected in a work of this nature. And superlative copyediting has given the volume a precision and polish that enhances its more substantive attributes. It is worth repeating that Hoppen's book sets a new standard of excellence in the study of Irish politics in its social setting.

JOHN D. FAIR
Auburn University

FRÉDÉRIC DELFORGE. *Les Petites Ecoles de Port-Royal, 1637–1660.* Foreword by PHILIPPE SELLIER. (Histoire.) Paris: CERF. 1985. Pp. 138. 159 fr.

In this book Frédéric Delforge assembles some elements for a historical understanding of the school of Port Royal. In his view the older studies devoted to Port Royal have become inadequate as a result of the more recent studies devoted to seventeenth-century spirituality. The work of Jean Orcibal, in particular, undeniably deepened our understanding of Jansenism. Thus, it seems plausible to attempt a new investigation of the school created by St. Cyran—a school in whose shaping such men as Antoine and Robert Arnauld, Antoine Le Maitre, and possibly even Blaise Pascal had a hand. That Jean Racine, Sebastien Le Nain de Tillemont, and Pierre du Boisguilbert were among the school's graduates is a further incentive for focusing on this peculiar boarding school.

Unfortunately, as Delforge is the first to recognize, the sources available for such an inquiry are as elusive as the school itself was ephemeral: it functioned only for twenty-three years (1637–60). Only a handful of students were in attendance at any given time, and the school's constant relocation was not conducive to record keeping. Few archival sources are available. Delforge establishes lists of pupils (about one hundred names) and masters (twenty-seven) whose names appear in later memoirs and reminiscences.

The program of study at Port Royal cannot be established. The best that can be done, and Delforge does it, is to infer as much as possible from pedagogical texts published after the school was closed by men who, in their youth, had participated in the enterprise.

The results of Delforge's inquiry can hardly be expected to surprise us. Port Royal appears as a small, expensive, sectarian boarding school with an excellent teacher-pupil ratio. The teachers were not very different from those who officiated in municipal public high schools such as the one in Beauvais, with which Port Royal maintained close links. The students came from a rather homogeneous social milieu: the world of officeholders. It is doubtful that any particular pedagogical innovation can be claimed for Port Royal. The emphasis on the French language and the admiration of Greek were common in many French schools in the sixteenth century. Erasmus appears as the *maître à penser* at Port Royal.

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If anything, Port Royal's originality seems to have resulted from its clinging to the earlier and higher standards of the great Renaissance educators at a time when Jesuits and Oratorians were cutting corners by increasing class size and neglecting Greek. The Jesuits were already marketing education as an instrument for worldly advancement. Port Royal, one guesses, stuck to philology and reflection.

GEORGE HUPPERT
University of Illinois, Chicago

HELMUT KÖTTING. *Die Ormée, 1651–1653: Gestaltende Kräfte und Personenverbindungen der Bordelaiser Fronde*. (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte E. V., number 14.) Münster: Aschendorff. 1983. Pp. 288.

The most radical subdivision of the Fronde occurred in the strategic city of Bordeaux during a war between France and Spain in which both states were vying for an alliance with Cromwell. Inevitably, the *Ormée* attracted widespread attention and provoked hundreds of the more than five thousand *mazarinades* that were published during the Fronde. In the past generation the variety of approaches to the events of the *Ormée* could well serve as a showcase for the assortment of interpretations—social, political, economic, ideological, or any combination of these—available to historians of early modern France.

Helmut Kötting's solid and heavily documented monograph exhibits a wide acquaintance with published source materials, including pamphlets, together with a scrupulous reading of previous scholarship on the subject. His supplementary archival research in England and France has served to clarify some disputed points and, occasionally, to broaden the debate.

After a succinct overview of the historiography of the *Ormée*, Kötting focuses on the city of Bordeaux—its role in the French tenth century. He next reviews the divisions that rent the city as the Fronde progressed from one stage to the next, its tangle of hatreds, fears, and ambitions fueling the last act of the rebellion.

The author clearly esteems the political agility and diplomatic finesse of Pierre Lenet, Condé's agent in Bordeaux, and has used the Lenet papers at the Bibliothèque nationale in Paris to good effect. Although lack of funds made Spain's intervention most unlikely, the multiple roles open to England were manifest. Kötting devotes considerable space to the activities of English agents in France, to the embassies in London representing Bordeaux, Condé, and Mazarin, and to contemporary speculation as well as evidence concerning English interest in the *Ormée*. Here he profited from the work of other historians such as Philip A. Knachel, but he has sifted a great mass of documentation and has reached conclusions of his own that are nuanced and persuasive.

Throughout the rebellion there was a desire for increased control over finance and justice and greater accountability of public officials, especially regarding their control over money. Although there was talk of a "republic" in Bordeaux, such a development might simply have connoted a move toward more autonomy within the French state; Kötting notes that scarcely anyone thought of abolishing the monarchy (pp. 126, 133). The author generally supports the viewpoint of the Mousnier school—he frequently refers, for example, to the work of Yves-Marie Bercé—as opposed to a Marxist-oriented perspective.

The extensive notes include many excerpts from the pertinent secondary works, including useful analyses from the unpublished master's thesis of Eckart Birnstiel (Berlin, 1977). While Kötting's book was in press, Christian Jouhaud published an absorbing essay on the *mazarinades* connected to the *Ormée* (*Annales, E.S.C.* 38 [1983]: 42–64). The interested reader will want to consult this article and its accompanying notes.

Kötting's study has a voluminous bibliography and a serviceable index. It will be of primary interest to those specializing in seventeenth-century France, the foreign and economic policies of Cromwell's regime, or insurrections and rebellions in early modern Europe.

JOHN T. O'CONNOR
University of New Orleans

JULIUS R. RUFF. *Crime, Justice, and Public Order in Old Regime France: The Sénéchaussées of Libourne and Bazas, 1696–1789*. Dover, N. H.: Croom Helm. 1984. Pp. 211. $28.00.

To the ever-growing bibliography on crime and repression under the Old Regime Julius R. Ruff contributes this study of a portion of Gascony during the century prior to the French Revolution. It is based primarily on the examination of 1,141 criminal cases drawn from six five-year periods in two *sénéchaussées*. No reason is given for beginning in 1696 rather than earlier in the seventeenth century. The author makes it clear, however, that he is especially interested in following the evolution of crime during the eighteenth century in order to test the theses of Norbert Elias, Pierre Chaunu, Michel Foucault, and others who see that century as a major turning point in the history of crime. The heart of the study is found in the final three chapters in which Ruff sketches the anatomy of crime as revealed in his sample cases. He notes the surprising

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pervasiveness of violence throughout all social milieus, its seasonal nature (such crimes were most common in the warm months), its predominantly masculine character, its frequent link to markets and taverns and Sunday leisure. Compared to the twentieth century, repressive values were commonly inversed: theft was treated unmercifully, while "mitigating circumstances" could reduce the sentences for murder, assault, and rape. Equally at variance with modern industrial society was the general predominance of crimes of violence—compared to crimes against property—a pattern that obtained in Libournais and Bazadais throughout the eighteenth century and well into the nineteenth. In this sector of southwestern France, then, the eighteenth century was not a turning point.

Few if any of these conclusions are particularly new or unexpected. Other authors (such as Emmanuel Le Roy Ladurie and Gordon Wright) have already suggested that the evolution of crime patterns in the eighteenth century was regionally complex and characterized by continuity as well as by change. In any case, the overwhelmingly rural province of Gascony is a curious area in which to test a link—as the author claims to have done (p. 105)—between shifts in the character of crime and "nascent capitalism." Although he buttresses his arguments with ample tables and statistics, he recognizes that all of his figures may be vitiated by the problem of unreported crime. The difficulty is serious since the great majority of the population, the peasantry, tended to settle its own problems without recourse to royal justice and is thus, as Ruff points out, heavily underrepresented in all of the statistical tables. In addition, given the extraordinarily severe penalties for theft, it is possible that crimes against property were underreported.

In general, however, the book is structured less on the exploration of historical problems than on the application of an elaborate categorization of crimes as taken from Chaunu and Pierre Deyon—with one or a few brief examples given for each category. Although most of these classifications are straightforward, one may question the grouping of crimes of physical violence (murder, assault, and so forth) with those of "verbal violence" (insults, threats, slander). Such a combination prevents Ruff from noticing the apparent increase of "verbal violence" in relation to physical violence in the course of the century (from 17 percent of all such crimes at the beginning of the century to almost 40 percent after 1707 [calculated from table 1.01]). This trend alone would have merited extensive commentary. In sum, the book is valuable as an additional case study and confirmation of earlier findings, but it is somewhat disappointing in its overall execution.

TIMOTHY TACKETT
Catholic University of America

WILLIAM M. REDDY. *The Rise of Market Culture: The Textile Trade and French Society, 1750–1900*. New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1984. Pp. xii, 402. $37.50.

This book is labor history in the mold of E. P. Thompson: it contains much rhetoric and moral fervor but little (and faulty) rigorous analysis and, for that matter, relatively little factual history. William M. Reddy purports to trace in the French textile industry the "transition to mechanized production and all its attendant cultural and social changes" (p. x). Although he used primary sources from the departmental archives of the major textile districts of northern France as well as from the Archives nationales and the Bibliothèque nationale, his book is not dense history in the manner of a French dissertation. Instead, it sketches various episodes of industrial conflict from the mid-eighteenth to the early twentieth century, interspersing them with many fatuous generalizations about human nature, motivation, and behavior. Its main theme, frequently but confusingly restated, seems to be that "a disjuncture between perception and reality" (p. 1) brought about by a new vocabulary—that of economics—misled both historical actors and subsequent historians into believing that the relations between production and the distribution of income were determined by free or open markets. "But this never really occurred" (pp. 1–2). Furthermore, "there is very little dependable knowledge of what the actual material conditions then were. Nor will such information become available until the whole accretion of pseudo-knowledge of economic conditions couched in the misused and misleading categories of market culture has been jettisoned" (p. 15).

Reddy claims to have applied the methods of cultural anthropology and economics to his historical data. I will not pass judgment on his use of anthropological concepts, except to say that they do not convince me. But his misunderstanding of elementary economics is profound. Economists use models of "perfectly competitive markets," but few if any believe that those models are accurate descriptions of reality. They merely isolate salient features of actual markets that are more or less competitive according to circumstances. Reddy discovers in the "economic man" of abstract theory a straw man with whom he proceeds to joust at great length until he declares himself the winner.

Reddy exhibits a penchant for grandiose generalizations devoid of apparent meaning and a tendency to infer motivation without evidence. Both are exemplified in the following sentence: "Because it is impossible to disprove an assumption about basic human motives, it is easy to find evidence for any claim—including the one made here, that the diffi-

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culty of interpreting human motives was a central determinant of certain behavior in the past" (p. 187). In the end, it seems, all the problems of society, past and present, boil down to a question of semantics, as suggested by the book's concluding sentence: "Only conscious reflection on the limitations of market language can provide the starting point for a new, genuinely free, social order" (p. 336).

RONDO CAMERON
Emory University

GUILLAUME DE BERTIER DE SAUVIGNY. *La révolution parisienne de 1848 vue par les américains.* (Commissions des Travaux Historiques, number 15.) Paris: Service des Travaux Historiques de la Ville de Paris. 1984. Pp. 239.

The Commission of Historical Studies of the City of Paris wisely chose Guillaume de Bertier de Sauvigny to compose a study of contemporary American sources reflecting views of the Parisian revolution of 1848. Although such a collection of documents in French translation is of value to French students of Franco-American relations, it conveniently serves American students as well.

The distinguished author concentrates on newspaper reports in New York, Washington, D.C., and New Orleans, three cities in which the unexpected revolution evoked fascinating discussions. In a fourth part individual American witnesses describe, often eloquently and penetratingly, what they observed in Paris between February and July 1848. The author lets these contemporaries speak for themselves: except for brief factual introductions and conclusions, he makes no elaborate evaluation or synthesis. The variety of substantive comments contained in his selections, however, offers the reader enlightening insights.

On the whole, Americans welcomed the proclamation of a republic in France as a moral triumph over political corruption and usurpation of power. They were proud that the success of their own system served as a model not only for France but also, they hoped, for the rest of Europe. Indeed, Irish-Americans liked to interpret recent developments in France as a catalyst likely to further the cause of liberty in Ireland. Despite doubts about the viability of republicanism in France, Americans felt encouraged by the initial moderation of the revolution. According to several contemporaries, they showed more interest in the turn of events in France than in the Mexican War.

Although the provisional government could see some advantage in America's moral support, some observers noted the reserve with which it was received. In fact, the original objective of French revolutionary leaders was to establish a republican system going far beyond that existing in the United States. Whereas Americans believed that government is best when it governs least, the French republicans promised to assume an active direction of social and economic affairs.

Indications of socialist tendencies among the liberated workers frightened French shopkeepers and the upper classes. When the gradual deterioration of conditions raised the specter of economic breakdown, anarchy, and war, Americans were disappointed. In case of war, they generally supported neutrality; military assistance was out of the question. But the growing troubles in France were expected to benefit the United States in two significant ways: first, Europe was unlikely to be able to influence Mexico's final peace terms; second, its capitalists would seek financial security by transferring funds overseas.

Among other extraneous aspects, the author pays much attention to newspaper and congressional reaction to the revolutionary government's emancipation of slaves in French colonies. In this often-heated debate between abolitionists and defenders of slavery the question of French republicanism assumed a subordinate role. The impact of the revolution on American material interests clearly caused more concern than ideological preferences. The exploitation of the events in France by political antagonists in the United States was apparently taken for granted.

HENRY BLUMENTHAL
Rutgers University

CLAUDE BRESSOLETTE. *Le pouvoir dans la société et dans l'église: L'ecclésiologie politique de Mgr Maret, doyen de la faculté de théologie de Sorbonne au XIX^e siècle.* Foreword by JACQUES GADILLE. (Histoire des Doctrines Ecclésiologiques.) Paris: Cerf. 1984. Pp. 211. 99 fr.

"Full of good intentions, he is not made for great struggles." So wrote the philosopher François Huet of Henry Maret, bishop *in partibus* of Sura and doyen of the Faculty of Theology at the Sorbonne from 1853 until his death in 1884. This opinion is now contested by scholars in France, Italy, England, and the United States. In 1977 Claude Bressolette's *L'Abbé Maret: Le combat d'un théologien pour une démocratie chrétienne, 1830–1851* decisively established Maret's importance as a theologian and democrat. His new book accents the centrality of Maret's ecclesiology as the foundation of his conviction that the church, by adhering to its fundamental nature, could be a more open society in harmony with the democratic and social aspirations of modernity.

According to Bressolette, Maret's philosophy of history, interpretation of the revolution, and con-

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ception of Christian civilization sustained a perception of the church as intrinsically and historically committed to man's freedom and dignity. He believed that sensitivity to democratic values required the hierarchical church to be responsive to all of its people and to the collegial rights of its bishops while retaining its form of government. Bressolette gracefully and elegantly explicates the logic, coherence, and articulation of Maret's vision, which was shaped by the disputes and divisions among nineteenth-century apologists, traditionalists, conservatives, and democrats. Historians interested in the origins of liberation theology may find the Aristotelian rigor of Maret's courses at the Sorbonne inaccessible and limited to the controversies of his time. But they will not fail to appreciate the intensity and historical significance of his effort to create a prescriptive theological discourse requiring democratic behavior within the church to match this desired end in society.

Valuable annexes to this volume document the concrete character of Maret's ecclesiology and engagement. Of particular importance is his letter of October 1852 to the archbishop of Paris, Marie-Dominique Sibour, urging him to vote to reject the Senate's intention to establish the Second Empire. "The empire," he wrote, "is absolute and *despotic* power with all of its deplorable consequences" (p. 179). France, he argued, is not so debased, degraded, and servile as to desire this dismal future. He admitted that the greater part of the clergy supported the establishment of the empire. The results would be that those who loved learning and liberty would renounce their faith; Catholics who supported the empire would prove the insincerity of their claims to love liberty, allowing materialist socialists and atheists to gain from such deplorable conduct. Maret insisted that the departmental councils had indeed welcomed the existing powers of the prince-president but that only half of them desired the empire. Members of the enlightened classes, he believed, preferred to keep the republic with its president for life, and he dismissed popular support for the empire as the action of the deceived, to be ignored by wiser legislators. Surprisingly, Maret in time came to admire and cooperate with the Second Empire. Bressolette proposes that this paradoxical action demands further historical attention. One hopes that he will give us that book.

EDWARD T. GARGAN
University of Wisconsin

MICHAEL BURNS. *Rural Society and French Politics: Boulangism and the Dreyfus Affair, 1886–1900.* Princeton: Princeton University Press. 1984. Pp. xi, 249. $26.00.

Some scholars contend that the peasant is a universal type and that rural societies display strikingly similar traits everywhere. Others argue that what small French villages have in common is an intense preoccupation with local problems, that every hamlet is therefore unique and should be examined separately. In this delightful and fascinating study Michael Burns has attempted to avoid these extremes by constructing a model on the framework of four French departments: the Gers, Orne, Isère, and Marne. Geographically scattered and demographically diverse, these areas are intended to provide a representative sample of agricultural communities in which to measure the seismographic impact of Boulangism and the Dreyfus affair.

The resulting volume is slender, concise, astutely written, and witty. It constitutes an extended essay whose purpose is to be suggestive rather than definitive. Burns has extensively probed in Parisian and provincial archives, yet he does not overstrain the evidence whenever, as often, it is skimpy or opaque. And he necessarily leaves the reader wondering whether the outcome would have been altered had another quartet of departments been chosen. For better and worse, then, the model is the method.

Burns is at his best in charting the reception of the Boulanger crisis in rural France. He assumes, quite plausibly, that the historian's business is to approach the theatrical aspects of the case, not to avoid them. His most dazzling pages are devoted to the propaganda campaign mounted in behalf of Boulanger by agitators of patriotic groups that boosted his candidacy. The illustrations are apt, and the description of the abundant iconography of the late 1880s is beautifully accomplished. Burns is able to show how French politics was "Americanized" (a contemporary term), thereby setting an enduring precedent for the twentieth century. He discovers that the general was soundly defeated in two (Isère and Gers) of the four departments studied; in the third (Orne) his campaign was preempted by a local notable; and in the fourth (Marne) he managed to win in only 81 of the 662 communes, largely because the population there was fearful of Boulanger's Germanophobic revanchism rather than attracted to it. For the most part the peasantry remained uninterested. Accordingly, Burns concludes that Boulangism had significance outside Paris and other urban centers only when it coincided with local preoccupations.

Somewhat the same may be said of the Dreyfus affair, but here Burns's model is inadequate, because enough evidence of a widespread response among rural folk is lacking. He therefore ventures randomly and cautiously, and his prose becomes more impressionistic. Anti-Semitism, he notes, was confined mostly to areas with a significant Jewish population and did not produce beyond those limits any

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important spate of propaganda. Despite some rioting in urban centers such as Grenoble, village life throughout the decade was marked mostly by indifference and the absence of effective organization on either side of the Dreyfusard debate, attributable in great measure to the general improvement of agricultural conditions. Burns thus rejects the notion that French racism was raised to the level of a coherent political movement or that the entire French nation was passionately divided by the Dreyfus controversy.

In the course of his analysis the author is torn between his mentor Eugen Weber, who argues that most peasants did not really become Frenchmen until the Belle Epoque, and his friends, especially Ted Margandant and John Merriman, who counter that the politicization of the rural populace must be placed at least a half-century earlier. Burns recognizes this altercation but hesitates to resolve it. Furthermore, he hardly mentions the war of 1870, which touched more French peasants directly and unavoidably than any other event in the nineteenth century; Léon Gambetta and Jules Ferry are scarcely noticed. Religious revivalism does not figure importantly either, nor do its anticlerical counterparts in the sharp conflict over public schools in the 1880s. Such vital links in the evolution of popular consciousness from 1848 to 1914 will need to be more firmly connected if we are to achieve a broader comprehension of French rural politics. Meanwhile, Burns deserves compliments for illuminating that problem and for treating two of its key episodes with deftness and lucidity.

ALLAN MITCHELL
University of California, San Diego

JEAN-DENIS BREDIN. *L'affaire*. Paris: Julliard. 1983. Pp. 561. 90 fr.

French socialists of the *fin de siècle* rallied late to the cause of Captain Alfred Dreyfus. Clinging to notions of Jews as rapacious capitalists, most socialists considered the scandal bourgeois folly. Only after Dreyfus had suffered on Devil's Island for three years did the quiet persuasion of Lucien Herr and the fiery eloquence of Jean Jaurès help mobilize socialists for this grand struggle in the name of justice and civil liberty. Now socialists of the new *fin de siècle*, careful not to repeat the tardiness of their ancestors, have rallied early to an unofficial centennial celebration of the heroic battles of the Belle Epoque. Max Gallo has written a biography of Jaurès, and Jean-Denis Bredin, an attorney active in the cultural affairs of the current government, has added his contribution to the nearly one thousand titles in the bibliography of the Dreyfus case.

Divided into four parts, the book moves chronologically through the anti-Semitism of the late Boulangist years to Dreyfus's arrest, conviction, deportation, retrial, pardon, and acquittal in 1906. In an epilogue summarizing the conflicting interpretations of the case, Bredin's powerful descriptive narrative gives way to an analysis of the long-term impact and current relevance of the affair.

With a lawyer's expertise, the author has reviewed judicial records at the Archives nationales and has studied the "secret dossier" at the army archives. His account, however, relies for the most part on published works: Joseph Reinach's seven-volume history of the affair; the memoirs of Alfred Dreyfus and his brother, Mathieu; and secondary works by Marcel Thomas, Michael Marrus, Zeev Sternhell, and others. Bredin gives us not only new evidence but also a new and thorough synthesis of the affair. Dramatic and detailed, this book is the best of its kind, as readable, if not as cogent, as Douglas Johnson's excellent but dated survey and more comprehensive than Thomas's widely read study.

Occasionally, Bredin's portraits of the affair's main actors become caricatures and his drama melodrama. But so it was during this bizarre scandal, which was marked by lofty ideals and gross perversions. To capture that high drama and low life Bredin had only to tell the story, and he has done so with style.

But the book is not without problems. Bredin reproduces copious documents when one or two examples would suffice; footnote numbers are confusing and many quotations are not attributed; and, finally, Bredin is wrong to suggest that French Jews enjoyed a "tranquillité rarement troublée" (p. 31) during the early nineteenth century. On the contrary, Dreyfus's family witnessed renewed anti-Jewish violence in the Alsatian countryside in the 1820s and 1830s and may have fled to the textile city of Mulhouse as much to escape persecution as to seize new opportunities.

On the years of the affair, however, Bredin is superb. For the first time we have a history that covers the case without ignoring its central figure. Bredin analyzes Dreyfus's notoriously rigid manner, fervent patriotism, and strict adherence to the military code of honor. But, had the author examined additional unpublished sources, he might have revised his belief that Dreyfus had no "sympathie spontanée pour ceux qui sont différents, étranges, étrangers" (p. 452). Following the affair, Dreyfus, still a close friend of Jaurès's, spoke at a number of trade union meetings; he did not always endorse the workers' tactics, but he had sympathy for their plight. Later he rallied to Sacco and Vanzetti, and shortly before his death in 1935 he supported his wife, Lucie, in her efforts to aid Polish Jews in Paris. He did all this quietly and with the obsession for

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privacy that Bredin describes well in this vivid account of the dozen years of the Dreyfus affair.

MICHAEL BURNS
Mount Holyoke College

LAURENCE K. SHOOK. *Etienne Gilson.* (Etienne Gilson Series, number 6.) Toronto: Pontifical Institute of Mediaeval Studies. 1984. Pp. x, 412. $25.00.

BERNARD E. DOERING. *Jacques Maritain and the French Catholic Intellectuals.* Notre Dame: University of Notre Dame Press. 1983. Pp. x, 268. $22.95.

The melting away of a distinctive Roman Catholic religious culture since the 1960s has encouraged us to forget the role played by the great "Thomists" Jacques Maritain and Etienne Gilson in earlier North American Catholic orthodoxy. Both of these men came to Saint Thomas Aquinas's cathedral of ideas in a paradoxical way.

Etienne Gilson, brilliant but of untroubled faith, redirected his passion for the exotic novels of Pierre Loti and Wagnerian operas to a love of the Middle Ages. Partly through the influence of Marc Bloch and Lucien Febvre this became centered on grasping the *mentalité* of its great figures: Saint Thomas, of course, but also Abélard and Héloïse, and Bernard of Clairvaux. A *bon vivant* and Catholic bourgeois, Gilson did not pretend to be more than a historian ("the only task of history," he wrote, "is to understand and to make understood, whereas philosophy must choose"). Laurence K. Shook marvels at how quickly the young scholar demonstrated a capacity for conveying certain basic intuitions of the "medieval mind" to others.

The young Maritain was the grandson of Jules Favre, a founder of the Third Republic. Rejecting his Protestant background, he came to demand the absolute truth, which involved a suicide pact with his fiancée, Raïssa Oumansoff. Bernard Doering chooses to deemphasize the famous "conversion" of Jacques and Raïssa in the lurid context of religious experimentation of turn-of-the-century France. He passes over their pilgrimages to the scatological prophet Léon Bloy in his hovel on Montmartre and to the Marian apparition site at La Sallette, as well as their decision to embark on a *mariage blanc*. Rather, in frank fidelity to Maritain, Doering launches extensive attacks on former Maritain friends who charged that Maritain was drawn to Catholicism by his affinity for the authoritarian dogmatism of the Action Française.

These intellectual biographies reveal that the "intuitionist" philosophy of Henri Bergson was decisive in the intellectual development of both men. It would have been useful if the authors had reflected precisely how this Jewish philosopher—who was often accused of irrationalism and whose writings were placed on the *Index Librorum Prohibitorum*—decisively influenced these twin pillars of orthodox scholasticism. Whereas Shook ascribes Gilson's successes at Harvard, Virginia, and Toronto to his wide learning and original scholarship, Doering attributes the apotheosis of Maritain among American Catholics and at the Universities of Chicago and Princeton to balanced and lucid social and political thinking. Why did these "Thomists" achieve greater prestige among North American Catholics than among their own countrymen? What was the context for that Gilson panegyric to the medieval mind in the William James lectures in 1937 or in occupied Paris? Why was Maritain, after a prominent role among the Free French, ostracized by French Catholics after World War II? Many of the more delicate and controversial topics are passed over out of deference to Maritain and Gilson. Both biographers chronicle the words and actions of these two distinguished men but are influenced by their subjects' own later reconstructions of what these all had meant.

Both of these books suggest that the French Thomists achieved the prestige and influence in North America that their learning and intellectual creativity merited, but neither tells us why its hero went so quickly out of fashion after Vatican II. They cite the disillusioned, sometimes bitter, reflections of these great men over the sudden vanishing of that Catholic philosophical culture they had labored to create—the displacement of the study of the great medieval texts by distressingly shallow, but native-grown and socially concerned, "new" theologians. They lived to see what Nietzsche observed of the Reformation: "the victory of something simple over something complex."

The Gilson biography is a thorough, genuine contribution to the history of both religion and philosophy in North America. Doering is particularly strong on Maritain's political thought. Neither book explains why these brilliant scholars were able to stimulate a passion for medieval philosophical texts or why that effort suddenly, in their lifetime, seemed dated and irrelevant. Both biographers, particularly Shook, successfully depict great, self-consciously Christian, scholars. But, as these men emerge in the expositions, they might as well have taught theoretical physics as the thought of Thomas Aquinas. It is unlikely that Princeton will again harbor a political philosopher who went on pilgrimage to Notre Dame de La Sallette or Harvard lionize a historian who matter-of-factly remarked that he regularly prayed to John Scotus Erigena and would settle a puzzling question with Ockham "when I meet him."

JOHN HELLMAN
McGill University

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MADELON DE KEIZER. *Appeasement en aanpassing: Het Nederlandse bedrijfsleven en de Deutsch-Niederländische Gesellschaft, 1936–1942* [Appeasement and Adaptation: The Dutch Economy and the German-Dutch Society, 1936–42]. (Cahiers over Nederland en de Tweede Wereldoorlog, number 7 and 8.) The Hague: Staatsdrukkerij. 1984. Pp. 234.

In the 1930s the German Foreign Office stimulated the founding of societies whose task it would be to improve relations with other European countries—Scandinavia, Great Britain, France, Poland, and Belgium. It was hoped that in those countries representative personalities, willing to organize national societies able to act as counterparts to German institutions, could be found; in Britain and France this initiative was successful. The Anglo-German Fellowship (1935) and the Comité Franco-Allemagne were prestigious organizations. In 1936 the Germans founded the Deutsch-Niederländische Gesellschaft. Its chairman was Emil Helfferich, a Hamburg businessman who had long worked in the Dutch East Indies and who was, though not a devoted Nazi himself, deeply committed to furthering the economic interests of the Third Reich. He successfully used his contacts in Holland to establish a Dutch counterpart to his society.

The scheme, however, was far less ambitious than that pursued in France and Britain. In the Netherlands there was no question of founding a formal society, nor did Helfferich look for Dutch representatives working in the fields of culture and politics. From the beginning he planned merely to gather a group of about twenty Dutch captains of industry who would discuss economic matters. From 1936 to 1940 the Dutch and German gentlemen met regularly and, over lunches and dinners, exchanged views about the urgent and extremely difficult economic questions facing both countries. Their dislike of the Nazi regime notwithstanding, the Dutch participants felt obliged to carry on business as usual for economic reasons; moreover, they hoped that economic appeasement would in the long run mitigate German political ambitions and thus contribute to political appeasement.

In this short book Madelon de Keizer studies the history of these contacts in great detail. They were not in themselves of major importance. Fully aware of this, the author did much more than merely chronicle a series of meetings. She defines the intentions of the Germans who took the initiative and analyzes German-Dutch economic relations. Her second objective was to sketch the biographies of the influential Dutch businessmen who responded positively to the German invitation without fully realizing that they were ultimately to be used as instruments in German propaganda campaigns. Finally, she describes the collapse of Dutch society during the German occupation. She has a firm grasp of her material and a fine feeling for the circumstances. Her judgments are perceptive and measured. This is a most welcome and enlightening book.

E. H. KOSSMANN
University of Groningen

GERHARD HIRSCHFELD. *Fremdherrschaft und Kollaboration: Die Niederlande unter deutscher Besatzung, 1940–1945*. (Studien zur Zeitgeschichte, number 25.) Stuttgart: Deutsche Verlags-Anstalt. 1984. Pp. 311. DM 48.

The focus of Gerhard Hirschfeld's study is on the phenomenon of collaboration during World War II and on the German occupation policies to which collaboration was a response. Although he basically concentrates on the response of institutions and organizations, he avoids certain topics, such as military collaboration and the cooperation of the Jewish Council, and omits an explicit discussion of others, such as general popular attitudes, at least after the initial honeymoon of the occupation.

Given this somewhat narrow framework, Hirschfeld, after outlining basic German occupation policies, examines in detail the first eighteen months of the occupation, which saw the rise and fall of the Netherlands union movement. He points out correctly that during this time members of political parties, trade unions, and the press formed fundamental attitudes that later blossomed into full-scale collaboration. Critical of the collaboration of government officials and agencies (including the secretaries-general, the national and local administrative bodies, the High Court, and the police), he concludes that most of the institutions and organizations discussed in the book worked to a considerable extent with the German occupation authorities, and he implicitly condemns much of that collaboration. Like other authors before him, Hirschfeld acknowledges that the Dutch Nazi party (the NSB) made relatively few inroads on existing institutions and organizations because of its inability to furnish qualified personnel and because of the universally hostile attitude toward the NSB expressed by that section of the conservative Dutch establishment on which occupation authorities had to depend in their endeavor to exploit the country on behalf of the German war effort.

The present study, originally a doctoral dissertation at the University of Düsseldorf in 1980, bears some of the marks of that origin, such as a certain reliance on social science conceptions and terminology that tend to give the study a somewhat static character. Although Hirschfeld appears to reject the distinctions between ideological and nonideological collaboration made by L. de Jong and myself (p. 8),

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he actually concedes in his final summation that the NSB and other "fascists" were the only Dutch people to collaborate on "political" grounds. In fact, Hirschfeld's implicit moral condemnation of collaboration is strong and consistent. Some readers will miss a somewhat more differentiated perception of the attitudes and actions of participants who did not know in 1940 and 1941 the shape of the future as we do. Even though no new startling conclusions emerge, however, the book will be useful (particularly to scholars who do not read Dutch) because the author has engaged in extensive research and read widely. His footnote apparatus occupies one-third of the total book. He used materials that have become available over the last three decades, and his judgments on the whole are sound and balanced.

WERNER WARMBRUNN
Pitzer College

W. D. J. CARGILL THOMPSON. *The Political Thought of Martin Luther*. Edited by PHILIP BROADHEAD. Foreword by A. G. DICKENS. Totowa, N.J.: Barnes and Noble or Harvester, Sussex. 1984. Pp. xi, 187. $26.50.

"He whom the gods love dies young!" (Menander). When W. D. J. Cargill Thompson passed away unexpectedly at the age of forty-seven, he left behind a manuscript on a subject long central to his intellectual concerns: Luther's political thought. Carefully edited by Philip Broadhead, who in lieu of footnotes also added the relevant bibliography for each topic, the book appears now as a significant contribution on a challenging subject.

The formidable literature on this subject is largely the work of German scholars of the stature of Heinrich Bornkamm, Gerhard Ebeling, and Werner Elert. But Thompson's book is more than merely "the best book in English," for Thompson rightly demonstrates that Luther was not an impulsive, erratic figure, as R. H. Tawney presented him, nor was he indifferent to political affairs, as J. W. Allen depicted him. Rather, he was a profound theorist on man and society who was for decades involved in the realities of politics. This book offers a dimension of understanding that goes beyond even Quentin Skinner's admirable treatment of Luther, because Thompson understands well the deep theological presuppositions from which Luther's political thought derived. For Thompson the focal point is the complex of ideas that form Luther's conception of the two realms, spiritual and temporal, corresponding to man's two natures and the eschatological doctrine, of biblical and Augustinian derivation, of the conflict between the kingdom of God and the kingdom of Satan. A judicious, subtle, and sound historian, Thompson grasps the complexity of the interplay of these dualisms and does not oversimplify or absolutize one theme such as "man between God and Satan" without giving full weight to "God's sovereign rule over both kingdoms." The author believes that Luther's principles were in place by 1521 and were given their first mature formulation in sermons of 1522 and the treatise "On Secular Authority" of 1523. Precisely because the author has a sound understanding of Luther's theology, he is able to relate his political thought to the religious dimension. Thus, his treatment of natural law and civil laws, political obedience, war and peace, church and state, toleration and various social questions not only reflects the very best in Luther scholarship but also is true to the texts and Luther's thought embodied there.

Luther's influence on political thought, the author observes, was far greater in the sixteenth century than Machiavelli's. Luther's eventual pronouncements on resistance to the emperor by the princes as inferior magistrates went further than Calvin's. And yet the author's suggestion that Luther believed that the common people had the right and duty to resist not only the pope but also the emperor is debatable. There are other infelicities such as an inappropriate comparison with Savonarola. No one can take a cheap shot at this author by labeling his work "an encomium," for he points to the limitations Luther set to religious toleration; the difficulties resulting from his use of the same terms in varying ways; Luther's justification for shifting his position, which sometimes amounted to a rationalization; and the difficulties his theory encountered in the face of harsh political realities. With this book Thompson has bequeathed to the scholarly world a most valuable legacy.

LEWIS W. SPITZ
Stanford University

PHILIPPE DENIS. *Les églises d'étrangers en pays rhénans, 1538–1564*. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 242.) Paris: Belles Lettres. 1984. Pp. 696.

In this splendid study Philippe Denis elects to study closely the type of church the earliest Protestants sought to organize. To find some particularly pure examples of the type, he limits his investigation to churches established by groups of refugees from Catholic oppression creating new institutions in sympathetic but relatively neutral surroundings. And to make his investigation manageable he concentrates on the fourteen churches created by groups of French, Dutch, and English refugees in cities and villages of the German Rhineland between 1538 and 1564.

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The book is symmetrically divided into two parts. In part 1 Denis provides capsule histories of the fourteen churches. The most important were those established by John Calvin in Strasburg and by John Knox and his associates in Frankfurt, but there were many others. Each chapter begins with a skillful analysis of the political and social conditions of the German community in which the new church was organized, followed by a meticulous narrative of that church's history from its beginning to its closing. These narratives are based on multiarchival and multilingual research, supported by an exceedingly close review of previous studies, that often points to omissions and errors on fine points of detail. As narrative histories they are likely to prove nearly definitive.

In part 2 Denis subjects this information to comparative analysis, which encompasses both the social and religious dimensions of the subject. He begins with analysis of the general characteristics shared by these fourteen churches. He then proceeds to a prosopographic analysis of the thirty-five ministers known to have served them. This is followed by chapters on the ways in which these churches sought to maintain discipline among their members, organize financial assistance to the poor among them, keep to a standard of doctrinal purity, and build reciprocal, supportive relationships with the neighboring churches. He finds Calvin to have been particularly influential on developments within almost all of these churches, not by virtue of any office or institution but through his skillful use of a network of acquaintances who retained enormous respect for his stature as an interpreter of the Word of God.

In conclusion Denis finds these churches all committed to a polity we would call congregational—fully autonomous, with the ministerial leadership elected and controlled by the general membership. He also finds them committed to a stance we would label confessional, maintaining precise and high standards of belief and behavior for all their members. But in the end he finds them ephemeral. Their commitment to high standards created internal tensions, and their separateness from the surrounding German Lutheran communities created external resentment. So almost all were closed within a generation, leaving models of considerable importance to later church development but not continuing communities of their own.

Altogether, this is a masterful contribution to ecclesiastical history, marking the emergence of a significant new talent.

ROBERT M. KINGDON
University of Wisconsin

BERND ROECK. *Reichssystem und Reichsherkommen: Die Diskussion über die Staatlichkeit des Reiches in der politischen Publizistik des 17. und 18. Jahrhunderts.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 112; Beiträge zur Sozial- und Verfassungsgeschichte des Alten Reichen, number 4.) Wiesbaden: Franz Steiner. 1984. Pp. xiv, 191. DM 54.

This book is an analysis of one of the most important themes of the theoretical and publicistic discussion of the Holy Roman Empire in the period from 1648 to about 1740. That theme, which to a large extent in these years (and even later) displaced the earlier and familiar debate concerning the form of government of this confusing political body, revolved around the very nature and character of the empire as a state—or, differently put, whether and in what sense the empire was a state at all.

Bernd Roeck contends that Samuel von Pufendorf was the chief catalyst in this shift of theoretical discussion. In 1667 Pufendorf characterized the Germanic body as an "irregular state-body, much like a monster" and suggested that it was at a midpoint of degeneration between a true monarchy and a system of confederated states. For Pufendorf's contemporaries and their posterity, the significant aspect of this formulation was both what it said about the form of government of the empire (namely, that it could not be comprehended within any of the classical, chiefly Aristotelian, categories) and, even more, its intimation that the empire had taken on some semblance of a league of states (*Bündnis*), which could mean that it was not really a state at all. Pufendorf did not intend this interpretation, and, indeed, most commentators on the imperial constitution, fiercely dedicated as they were to the proposition that the empire was a state, came to realize that Pufendorf himself had contributed a concept that could rescue the statehood (*Staatlichkeit*) of the empire. This was the concept of "system," which unlike *Bündnis(system)* was a term that could accommodate irregularity in the form of government of a political body without denying it identity as a single state. Thus, the term *Reichssystem* came into common usage as synonymous with *Reichsverfassung* and to a degree even replaced it.

But, as Roeck points out through a discussion of both minor and major publicists of the period, the search for an imperial unity necessary to true statehood in the context of the rapid diffusion of political and constitutional authority within the empire after 1600 led to an increasing emphasis on history as the means of establishing the identity of the empire, especially through display of the continuity of imperial public law. The historicity of the empire would anchor its identity as a state, and that historicity was uniquely (and perhaps only) demonstrable in law. But, since positive or statute law was fragmentary and contradictory, it was inadequate to this

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demonstration; thus, great reliance came to be placed on *Herkommen* (tradition, usage) and *Gewohnheitsrecht* (customary law) as the distinguishing (and peculiarly "German") earmarks of the imperial constitution. Although much subject to abuse, deliberate and otherwise, *Herkommen* came largely to define the legal order (*Rechtsordnung*) that gave the empire its character as a state until its dissolution in 1806.

Roeck's work—though frequently repetitious, longer than it needs to be, and stylistically dense—is nevertheless an interesting contribution to our understanding of the logic and loyalties of the defenders of the old empire.

JOHN G. GAGLIARDO
Boston University

LINDA FREY and MARSHA FREY. *Frederick I: The Man and His Times*. (East European Monographs, number 166.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. ix, 303. $28.00.

The first king of Prussia, Frederick I, has never been considered a powerful and successful monarch and has seemed to be a peculiar and unfortunate exception to a formidable list of rulers of outstanding ability and accomplishment ranging from the Great Elector to Frederick the Great. Long interested in the foreign policies of Frederick I, Linda Frey and Marsha Frey have now attempted a full-scale biography of the king. Although this book has certain value, it unfortunately is not a strong and convincing work.

The authors intend to rectify what they consider too extreme an interpretation of Frederick, but they succeed only to a limited extent. The book is divided into groups of chapters that consider the personality of the king, his administration, cultural and religious policy, and foreign policy. Pliable and agreeable, Frederick obviously liked a happy court. The Freys admit that he was particularly vulnerable to the machinations of cabals and the intrigues of ministers. A man of personal rectitude and real affection for his family, Frederick emerges as more humane than his awesome progenitors and successors. This portrait of the king is well known to specialists.

More dubious is the Freys' contention that Frederick consistently pursued a *Gesamtstaat* or *Staatsbildung* policy of administration. One wonders whether his weak personality traits ensured little continuity of policy. Certainly, the various ministers, such as Heinrich von Ilgen, Johann, Count Wartenberg, and Eberhard von Dancklemann, seem to have been men of greater will power and intelligence than their nominal master. Public administration during this period seems to have been a peculiar amalgam of vacillating policies, corruption, and high-level intrigue not controlled by the king.

Discussion of cultural policy is encyclopedic rather than analytical. Historians of the German Enlightenment have long been fascinated by a remarkable flourishing of scientific, architectural, literary, and musical endeavors in Berlin under the first king. Usually they assume that not the king but his second queen, Sophie Charlotte, was the leader and inspiration for Gottfried Wilhelm Leibniz and a host of others. The Freys do not clarify this question, leaving unresolved the role of Sophie Charlotte without illuminating that of her husband. The documents, as well as convention, seem to support the interpretation that the queen was the leader of culture.

More successful is the discussion of foreign policy matters, to which the authors devote the last substantial part of the book. In this field their touch is more deft and their understanding more complete. But even here the Freys are forced to admit that many of the king's policies were contradictory and even self-defeating. The authors do remark that the acquisition of Neuchâtel, Moers, and other territories added to the size of the realm. Frederick the Great, however, later saw the ownership of some such enclaves as a nuisance rather than a benefit. The authors claim correctly that the argument of the old Prussophiles—that great territorial gains might have been possible in the Great Northern War if the king had played a shrewder game—seems far-fetched. The acquisiton of part of Pomerania was perhaps the most that could be expected. Good use of archival sources strengthens the discussion.

As a biography this work is an elementary introduction to the person and his times. As a contribution to scholarship it illuminates Prussian foreign policy.

HUBERT C. JOHNSON
University of Saskatchewan

JOHN C. FOUT, editor. *German Women in the Nineteenth Century: A Social History*. New York: Holmes and Meier. 1984. Pp. xi, 439.

Like the recently published study *When Biology Became Destiny: Women in Weimar and Nazi Germany* (1984), this book by John C. Fout attests to the growth and development of the area of German women's history in the last decade. Not long ago this collection would have been thin and sketchy. But scholars working on both sides of the Atlantic have contributed to a body of literature that has made possible, and perhaps even necessary, such a substantial collection of essays.

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Fout's book contains original articles on a number of aspects of nineteenth-century German women's history. About one-fourth of the book is devoted to Fout's extensive historiographical introduction and an English-language bibliography. The articles themselves are arranged in two sections (although this is not indicated in the table of contents), with more than two-fifths of the book's pages devoted to middle- and upper-class women and about one-third to working-class women (or, more accurately, working-class women, female peasants, and women in domestic industry). The essays vary in their methods and sources, from quantitative analyses of rural infant mortality rates and female homocide rates to intellectual and psychological case studies of individuals such as Hannah Arendt, Henriette Schleiermacher, Kathinka Zitz-Halein, and Lily Braun. Fout's introduction is a historiographical gold mine and an indispensable guide to the literature on women's history in both German and English. It discusses the major works, comments on the sources used by historians, and points to gaps in the existing scholarship. But it is not really intended to pull out common themes in the collection; that is more or less left to the reader.

Fout does put his finger on a significant weakness in the literature on German women's history: the tendency for those interested in working-class women to focus on socialist women and the complementary tendency for historians of the middle and upper classes to emphasize feminist women. This pattern—which results in part from the kinds of available sources—is evident in this volume, although many of the essays also break away from it. There are fascinating pieces of both sorts: Deborah Hertz on Hannah Arendt's *Rahel Varnhagen*; Catherine Prelinger on the complex motivations of Amalie Sieveking, the founder of the Female Association for the Care of the Poor and the Sick and in many ways an interesting German counterpart to Catharine Beecher; Juliana Jacobi-Dittrich on middle-class girlhood; and Jean Quataert on the impact of social insurance on the work and consciousness of women weavers in south Oberlausitz, to mention only a few.

This is an important book because it makes accessible to scholars of German history and women's history a sampling of recent scholarship in the field. The articles address such major questions as the impact of industrialization on women's work and family life and the origins of the women's movement. The late and rapid industrialization of Germany and the weakness of German liberalism after the revolution of 1848 make the experience of German women in these two areas particularly interesting for women's historians working primarily in other national fields. Although the book is probably not as accessible to undergraduates as Fout hopes, it is an important resource for German and women's historians.

LEILA J. RUPP
Ohio State University

MANFRED SIMON. *Handwerk in Krise und Umbruch: Wirtschaftliche Forderungen und sozialpolitische Vorstellungen der Handwerksmeister im Revolutionsjahr 1848–49*. (Neue Wirtschaftsgeschichte, number 16.) Cologne: Böhlau. 1983. Pp. xvii, 652. DM 96.

Doctoral dissertations, even the ones good enough to merit publication, generally display more assiduity and perseverance than sharp analytical skill. Manfred Simon's book on the artisan movement during the German revolution of 1848–49 is a case in point. For almost 16 years Simon painstakingly examined the minutes of various meetings of handicraftsmen and the petitions that these artisans submitted to the successive political authorities in Frankfurt am Main: the Preliminary Parliament, the Committee of Fifty, the National Assembly, and the Economic Committee. To be sure, this vast body of material has not simply been gathering dust for the last 135 years. Other writers have looked at it, most notably, Paul Noyes in his book on working-class associations in the revolution. But no one has gone through it more methodically than Simon, who dwells in great detail on all the major themes and minor features of the artisan movement—its goals, demands, assumptions, predilections, and even its digressions and meanderings.

Yet, although the reader will find here everything he ever wanted to know about the subject, the conclusions that emerge are rather bland. The book is so full of caveats, provisos, exceptions, and reservations that no clear pattern is discernible. Simon challenges the contention of such earlier writers as Veit Valentin that the artisans were resolutely opposed to economic liberalism. And, in fact, he quotes some voices among the handicraftsmen in favor of free enterprise and industrial manufacture. Still, when he finally summarizes the "sociopolitical concepts and demands" of the artisans and examines "the master handicraftsmen and their relationship to the political events of the revolutionary year," the picture is not much different from that presented by other scholars. The artisans still oppose the advance of industrialization, they still favor some form of economic regulation that will enable them to compete with factory production, they still seek to preserve corporative guild authority, and they still find a traditional, hierarchical political order more congenial than a liberal, individualistic one. Simon modifies the details of some older accounts and tones down some of their more extreme assertions. But for the most part he does not stray

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far from the conventional view of the artisan movement.

The most useful and original part of the book is the long appendix of almost two hundred pages that enumerates and summarizes the petitions submitted to the National Assembly by artisan circles. Each item is provided with the identifying number by which it may be located in the Frankfurt archives, and its contents are summarized as well, sometimes with illuminating quotations. This appendix will prove valuable to any scholar doing work on the economic and social aspects of the revolution of 1848–49. Even those who might wish that Simon had been a little bolder in his generalizations or more venturesome in his conclusions will be grateful to him for performing a laborious and tedious but important task. This alone makes his book a useful addition to the scholarly literature on the German artisan movement.

THEODORE S. HAMEROW
University of Wisconsin

LUISE SCHORN-SCHÜTTE. *Karl Lamprecht: Kulturgeschichtsschreibung zwischen Wissenschaft und Politik.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 22.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 373. DM 96.

GANGOLF HÜBINGER. *Georg Gottfried Gervinus: Historisches Urteil und politische Kritik.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 23.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 257. DM 65.

These two books testify to a growing interest in past historians and historiography in Germany, a phenomenon that itself reflects an undoubtedly healthy breakdown of consensus over the proper objects, questions, and methods of the historian during the past two decades. Each monograph deals with a nineteenth-century historian who was provocative, innovative, and controversial in his own time and fell into that limbo occupied by defeated challengers of ruling paradigms and orthodoxies. For, however little Georg Gottfried Gervinus and Karl Lamprecht have in common, they certainly rejected the canons of *Historismus* that dominated the German historical establishment for at least a century after 1850.

Gervinus did more than reject the political quietism and "value-neutral" postures of his contemporaries the Rankeans. He was a revolutionary leader in 1848 and one of the most spectacular martyrs to survive the reaction of the 1850s. He was also a rare critic among historians of Bismarck's unification of Germany and a proponent of a democratic theory of historical development.

Lamprecht is most closely associated with *Kulturgeschichte*, by which he meant not the history of ideas (which was acceptable to historicism) but the history of material culture. According to Luise Schorn-Schütte, Lamprecht was a forerunner in the study of "mentalities." In the decades before World War I he fought a running battle against a German historical establishment scornful of "materialistic" explanations and consciously isolated from developments in the natural and social sciences, from which Lamprecht drew much of his inspiration.

Both monographs are meticulously researched analyses of the historical thought of each man rather than traditional biographies. Schorn-Schütte disposes of biographical information in the first chapter and then moves on to various topics: the "concept of *Kulturgeschichte*," forms of human socialization as an object of Lamprecht's study, his activities in cultural and scientific policy (for example, university reform), and his relations with the American New Historians, the French *histoire synthétique* movement, and other foreign historical circles.

Schorn-Schütte has further provided a useful service by combing Lamprecht's writings and piecing together a coherent description of his historical concepts and theories, including changes in them. She also defends Lamprecht against the charge of his German detractors that he was "merely" a representative of West European positivism, since many of his early ideas had roots in the German Romantic tradition. One weakness of her informative work, however, is the lack of thorough discussion of the motives and methods of traditional neo-Rankean historians in battling Lamprecht's view that history must become a social science and study the patterns and regularities in the past. This reactionary opposition and the resulting setback of German historical writing by decades is at least as important as Lamprecht's own suggestions.

Gangolf Hübinger's approach is chronological and topical, a choice that perhaps reflects Gervinus's more politically and journalistically active role as a noted liberal leader of the *Vormärz*. As the subtitle implies, Hübinger's somewhat obvious "thesis" is that Gervinus's historical judgment was influenced by his political engagement. Although this work does not fundamentally alter existing interpretations of Gervinus, it is based on an exhaustive study of Gervinus's remaining correspondence and includes a full bibliography of all of his published works. It is thus a useful addition to his intellectual, if not his general, biography.

CHARLES E. MCCLELLAND
University of New Mexico

ERIC DORN BROSE. *Christian Labor and the Politics of Frustration in Imperial Germany.* Washington, D.C.:

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Catholic University of America Press. 1985. Pp. x, 410. $34.95.

Germany's interconfessional Christian trade unions have been neglected by historians in favor of the much larger socialist free unions, and they have sometimes been dismissed as merely an appendage of the Catholic Center party. In fact, as Eric Dorn Brose shows, their leadership maintained links with most of the nonsocialist parties. Brose gives a good account of the origins of the Christian unions and their social philosophy; he is especially to be commended for his attention to the Protestant, Christian Social wing of the movement.

Brose's main concern, however, is to use the history of the Christian labor movement to illustrate and confirm his interpretation of the nature of the Wilhelmian regime and to "build on the spadework of [Richard J.] Evans, [David] Blackbourn, and [Geoff] Eley" (p. 17) in revealing how Christian labor leaders were manipulated and frustrated not only by the government and traditional elites but also by "auxiliary elites" like conservative party politicians and Catholic and Protestant clergy. The political goals of the movement—to build a coalition in the Reichstag favorable to social reform legislation and to introduce codetermination (*Mitbestimmung*) into industry—were consistently thwarted. But, by choosing to concentrate on the political activity of the unions and by neglecting their everyday economic activities at the local level, the author exaggerates their failures and underestimates their achievements. They were rebuffed by the upper classes precisely because they represented a genuine workers' movement not much less threatening than its socialist counterpart. A fuller and more balanced account of the Christian unions is provided by Michael Schneider's recent *Die Christlichen Gewerkschaften 1894–1933* (1982).

Brose concludes his study with the war years, when the Christian unions, having successfully weathered the long controversy within the Catholic community over the desirability of interconfessional unions, became more militant, demanding Prussian suffrage reform and cooperating for many purposes with the socialist unions. The general secretary of the union federation, Adam Stegerwald, hoped at the end of the war to use his organization, supplemented by social-minded Centrists, Conservatives, and disillusioned Socialists, as the basis for a realignment of the party system, building a new party of the "Christian middle." Brose believes this would have been a healthy, constructive development, reducing the excessive number of German political parties. But he fails to realize that the new alignment would have excluded the secular, liberal viewpoint that had formed the "middle" of the political spectrum. (The liberal Hirsch-Duncker unions dissolved their brief merger with Stegerwald's federation in 1919.) In Brose's opinion, Stegerwald's failure was "tragic for the young republic and certainly helped pave the way for Hitler's rise to power" (p. 370). This is an extraordinary statement, considering the antirepublican, near-fascist attitudes displayed by Stegerwald in the Weimar period and his obstinate rejection of any accommodation with the Social Democratic party. In any case, as the author recognizes, Germany's divisions of class, caste, and confession were still too firmly entrenched in 1918 to make any such realignment possible. The continued existence of three separate labor movements until their forcible dissolution in 1933 attests to this.

ELLEN L. EVANS
Georgia State University

ANN TAYLOR ALLEN. *Satire and Society in Wilhelmine Germany:* Kladderadatsch *and* Simplicissimus, *1890–1914*. Lexington: University Press of Kentucky. 1984. Pp. xiii, 264. $25.00.

In this thoughtful, well-crafted study, Ann Taylor Allen analyzes two of imperial Germany's most famous satirical journals, *Kladderadatsch* and *Simplicissimus*, as vehicles for understanding the evolution of the social and political attitudes of the liberal middle class in the Wilhelmine era. Through cartoon art, editorials, poems, and other devices, the older, more moderate *Kladderadatsch* (founded in 1848) and the younger, more radical *Simplicissimus* (founded in 1896) satirized disturbing and undemocratic features of Wilhelmine political life. In contrast to *Kladderadatsch*, which confined its satire mainly to public life, *Simplicissimus* also lampooned various aspects of private life, such as bourgeois marriage, the authoritarian German family, and the stultifying educational system. The popularity of both journals, but especially the dramatic success of the innovative, hard-hitting *Simplicissimus* after 1900, Allen argues, reflects "not only the disenchantment of a considerable segment of the middle-class public with a ruling elite whose prestige had been seriously eroded, but their tentative search for a new political ideology" (p. 227).

Allen skillfully contrasts and analyzes not only the differing political content and intended audiences of the two satirical journals but also their styles, verbal and visual imagery, and commercial bases. She makes good use of theories about the psychological and social function of humor, although she too often equates humor and satire. A wider familiarity with the work of literary critics such as Northrop Frye, Ronald Paulson, Philip Pinkus, and A. B. Kernan, who have examined satire as a definable style and literary genre and have analyzed the structure, images, and recurring symbols in satirical

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art, would have added an important dimension to Allen's work and placed her study within a larger context.

One of the central issues of the book is whether political and social satire creates or merely reflects public opinion. Allen convincingly demonstrates that *Kladderadatsch* and *Simplicissimus* gave expression to a rising current of progressive dissent in Wilhelmine Germany and that the two journals became an increasingly important vent for bourgeois protest. Such findings should lead us to reassess some of our assumptions about the supposedly complacent, "feudalized" German middle class. The question of how much the satirists actually shaped popular opinion and promoted middle-class disenchantment with the established order is a far more difficult one, however. Allen argues that, besides serving as a tension-releasing mechanism for repressed hostility and aggression, humor is also an active force for social change; for, by mobilizing latent rebellious tendencies, by promoting group cohesion, by creating and popularizing symbols that can shape political consciousness, and by providing a vocabulary through which dissenters can express their discontent, humor can be a causative factor in molding public opinion and challenging established power structures.

Although *Kladderadatsch* and *Simplicissimus* could have performed these functions in Wilhelmine Germany, it is not clear from the evidence Allen presents that, in fact, they did. Analyzing the impact of printed matter on readers is an enormously difficult, perhaps impossible, task, and conclusions about how opinions are actually affected by what people read or view can be little more than speculative. To argue, as Allen does, that the hostility and opposition these two journals aroused within the conservative governing elite is proof that they must have been exercising a formative impact on public opinion does not, in itself, seem persuasive. In short, Allen tends to overestimate the significance of satire as an active cause of those shifts in *fin-de-siècle* bourgeois opinion that she so deftly details.

Even if *Kladderadatsch* and *Simplicissimus* perhaps followed more than led the evolution of liberal perceptions in the Wilhelmine era, Allen has provided us with many new insights into the complex interaction of humor, politics, and society at a time when satire functioned as an important means of self-expression for elements of the German middle class.

GARY D. STARK
University of Texas, Arlington

DAVID N. SPIRES. *Image and Reality: The Making of the German Officer, 1921–1933*. (Contributions in Military History, number 38.) Westport, Conn.: Greenwood. 1984. Pp. xvi, 260. $29.95.

Whereas most historians of the *Reichswehr* have examined its political role or concentrated on details of armaments policy and strategic thought, David N. Spires looks at the selection and training of officers, their postings, promotions, dismissals, and operational training. His book is based largely on the records of the Bavarian Seventh Division and makes extensive use of the files of two officers, Ferdinand Jodl and Eduard Zorn.

Spires attempts to get around the question of the social composition of the Weimar officer corps by saying that officers tended to come from conservative sectors of society that traditionally supported the army. This does not, however, answer critics who pointed out that some 85 percent of the officers came from families of former officers, senior civil servants, academics, and professional men and that only 6 percent were the sons of factory owners and merchants. Although bourgeois officers scarcely differed from aristocratic ones in attitudes, the fact that about 21 percent of the officer corps were aristocrats (they were about 0.14 percent of the total population) is surely not without significance. Although the army preferred officers who had passed the *Abitur*, many were selected for "character," which all too often meant loyalty to Seeckt and to the *Reichswehr* rather than to the republic.

For the army, the main problem concerned two contradictory missions: to create an army that was combat ready and to undertake the training necessary for a vastly expanded "post-Versailles" force. The desire to create a large body of highly trained officers meant that individual units suffered from a quick turnover of talented officers, which affected combat readiness. Unfortunately, Spires does not examine the political background to this dilemma, so the whole question appears as little more than a technical difficulty.

Training was also affected by the size of the army. In such a small officer corps individual officers could only with difficulty be spared for training. Therefore, training programs were decentralized to minimize disruption, and they alternated with service in the line. The author insists over and over that the training itself was rigid and inflexible, producing a dull conformity and stifling individual initiative. Little evidence is produced to support this contention, and it remains doubtful whether any other army produced such a number of highly competent and effective staff officers.

Promotions and dismissals also presented problems. In the early years officers often remained captains for up to twelve years. Heye tried to weed out substandard officers, improve the officer corps, and attack the fossilized traditions and rigidity of the

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army, but his willingness to compromise with the republic came from weakness rather than conviction, for the army was under severe criticism from the Social Democrats, particularly from Scheidemann and Löbe. That this elimination program resulted in a marked lack of civil courage in the officer corps, as Spires contends, is questionable, and the army's ambivalent attitude toward the Nazis can hardly be traced back to this cause.

Spires concludes that the army had many weaknesses but also many virtues. Central control and individual initiative were balanced, training programs were effective, and advancement was based largely on merit. In spite of the degree of decentralization uniform standards were applied, and the professional army had surprisingly high morale in spite of all the difficulties. His conclusion that the decision to abandon the professional *Reichswehr* for a conscript army resulted from tensions within the army rather than from a deliberate political decision is highly dubious, as is his belief that a conscript army is necessarily less isolated from its social base than is a professional army.

There is much of interest in this study, but it is based on a rather modest number of sources, and some of the conclusions are not truly substantiated. To make the manuscript book-length, it is stuffed with undigestible tables of questionable value. The production is a disgrace. Typographical errors occur on almost every page, and whole sections of sentences are missing, thus rendering them meaningless. The author deserves better.

MARTIN KITCHEN
Simon Fraser University

ROLF-DIETER MÜLLER. *Das Tor zur Weltmacht: Die Bedeutung der Sowjetunion für die deutsche Wirtschafts- und Rüstungspolitik zwischen den Weltkriegen.* (Wehrwissenschaftliche Forschungen; Militärgeschichtliche Studien, number 32.) Boppard a/R.: Harald Boldt. 1984. Pp. xi, 403.

The patterns of thought and political expectations as well as the similarities in the structure of German decision making that unify the interwar years have their precedents in the Wilhelmine period. One might want to explore how these postwar "norms" fit the changing conjunctures of politics and economics, how deeply they penetrated into German society and who exactly embraced them, or how these standards of what after all was German bourgeois ideology were reshaped by war, revolution, and counterrevolution. But this is not what one can expect in the German context. There the debate centers around the issue of whether this kind of ideology existed at all and who among the leading politicians, industrialists, and officials should be counted among the adherents of power politics and, hence, must be considered a gravedigger of the first republic. It is small wonder that this is a principled debate.

Although it has always been argued that East Central Europe and the Balkans were targets of German expansionist ideas, Russia—and not just the border territories, but all of European Russia and possibly beyond—is of a somewhat different caliber. Rolf-Dieter Müller argues at least by implication that Russia was one of the main targets of German expansion. He shows that the use of Russian resources was considered to be key to a German economic and military recovery and, hence, the basis for future German economic power. Under these conditions, mere exchange relations—diplomatic negotiations and agreements, trade arrangements, and military relations—between the two countries did not suffice to satisfy the German elites, which maintained that, for Germany to regain world power status or European hegemony, Russia had to be subdued either indirectly through monopolistic exploitation or by military force.

This is not exactly what happened in the interwar years, and, when it happened in 1941, Müller freely concedes that German expansion was motivated by a different ideology. Arguing that this is what German elites wanted, he shows the thoughts and the long-term expectations behind political decisions and points to the pitfalls of the notion that considers exchange relations between an industrial nation and a nonindustrial nation as the "natural" form of international relations.

Ultimately, though, this study remains unconvincing, because it suffers from a general malady that has crept into the whole debate. Müller, like many others, never specifies the "leading circles in politics, the state apparatus, the military, and industry" (p. 343) to whom these ideas are attributed. These ideas—which were real enough and cannot be discarded or softened as the opponents of this school would like—are, in addition, generated from such a strange mash of sources that the whole scholarly endeavor is jeopardized. These weaknesses indicate a continuing inability to come to terms with the problem of ideology and power as well as of consensus building and decision making in German politics.

Nonetheless, Müller is right in emphasizing that there was a powerful German conviction not only that bolshevism had to be destroyed but also that the destruction of bolshevism would profit Germany by setting free resources, territory, and people for German exploitation. It was the author's choice to set this kind of conviction apart from an "irrational" and "dogmatic" racism of the National Socialists. It would not be mine; for both ideas are rational and

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explicable in their own contexts and both are certainly racist.

MICHAEL GEYER
University of Michigan

JEFFREY HERF. *Reactionary Modernism: Technology, Culture, and Politics in Weimar and the Third Reich.* New York: Cambridge University Press. 1984. Pp. xii, 251. $29.95.

Reactionary modernism is an ideal typical construct Jeffrey Herf devised to explain the cultural politics of those philosophical irrationalists and political antimodernists who paradoxically also embraced technological modernization. Certain conservative revolutionary intellectuals in Weimar, German engineers, and Nazis reconciled *Technik* and *Kultur* by separating technology from other aspects of modernism and by associating technology with *Geist* and with will rather than with materialism and capitalism. Reactionary modernists created an ideology of "steellike romanticism" in which technology, emanating from the German soul, was integrated with nationalism and provided the foundation for a powerful state. In doing so, they had selectively to accept part of Enlightenment rationality and modernity and to reject the pastoral utopian visions and antitechnological trends of traditional romanticism.

Challenging those who interpret Nazism essentially as an antimodernist revolt that later either abandoned ideology for power or used modern technology to implement antimodernist politics, Herf argues that ideology was politically decisive in the Third Reich and that Nazism was "by no means an unambiguous rejection of modernity" (p. 235). Technology was viewed as an inherently valuable manifestation of superior German *Kultur* and the National Socialist "spirit."

There are, however, cracks in the edifice of this ideal type. Although most of the book focuses on the conservative revolution, Herf admits that not all members of this movement accepted technology, and it is questionable whether four of the six figures he selects for detailed study fit his model. Whereas the chapters on Ernst Jünger and Hans Freyer strongly support his thesis, other sections conclude that Oswald Spengler, though not antitechnological, remained pessimistic about the future and technology and that Martin Heidegger not only rejected technology as irreconcilable with culture but also was not a reactionary modernist. Carl Schmitt is forced into this category only by strained, inaccurate, or misleading inferences drawn from limited sources. Despite Schmitt's devastating critique of romanticism, Herf erroneously states that Schmitt proclaimed himself a romantic who celebrated the romantic politician in the service of technological advances. The chapter on Werner Sombart deals primarily with anticapitalism and anti-Semitism and only peripherally with technology. Herf concludes that Sombart retained residues of ambivalence, lacked a philosophy of technology, and did not view technology as a product of will. Similarly, Herf establishes that some engineers synthesized will and nationalism with technology and that this facilitated their compromise with Nazism. Yet he concedes that the largest engineering association, like most engineers, was unconcerned with the cultural crisis and that, compared to other professional groups, engineers were underrepresented in the NSDAP. Herf also neglects the diverse, competing, and often contradictory currents among rightist intellectuals and in Nazism.

The essence of this interpretation is not new, as the literature on conservative revolutionaries, Karl-Heinz Ludwig's study of engineers, Walter Benjamin's work, and Thomas Mann's view of Nazism as the unity of dreams of the past and robust modernity all indicate. Moreover, whether the synthesis of *Technik* and *Kultur* was as influential in Third Reich politics as Herf contends is still open to discussion. Nevertheless, this book is a lucid account of the significance of ideas, and reactionary modernism is a useful concept for understanding certain thinkers and currents.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

EBERHARD JÄCKEL. *Hitler in History.* (Series from the Tauber Institute for the Study of European Jewry, number 3.) Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1984. Pp. viii, 115. $11.95.

In contrast to most scholarly works on Hitler, this brilliant book is lucid, forthright, and blessedly brief. Eberhard Jäckel wears his erudition lightly, and perhaps only a specialist can appreciate the formidable amount of learning that underlies this slim volume.

The book's title is somewhat misleading, for Jäckel wants neither to evaluate Hitler's place in history nor to analyze historical opinion about the Nazi leader. Instead, he attempts to answer a number of critical questions about how Hitler came to power, how he set about implementing his two major goals (the conquest of *Lebensraum* in Eastern Europe and the elimination of the Jews), how he came to declare war on the United States, and the nature of Hitler's relationship with the German people.

Jäckel reminds his readers that Hitler did not come to power through a coup d'état or a revolution but was appointed by Hindenburg. He maintains

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that, although Hindenburg's conservative entourage encouraged this appointment, Hitler was not the representative of any one class or group. The author believes that the controversy among historians about the nature of Hitler's authority (whether he was the supreme decision maker or whether decisions were made in response to conflicting forces within the state) is based on profound misunderstanding on both sides. "There is abundant evidence that all the major decisions in the Third Reich were made by Hitler, and there is equally abundant evidence that the regime was largely anarchic and can thus be described as a polycracy. . . . [But] polycracy was the very condition of monocracy, for the monocrat maintained his power by playing the conflicting groups against each other" (p. 30).

Jäckel is convinced that Hitler had already formulated all major political objectives before he came to power and that without understanding these it is impossible to evaluate how he prepared for, initiated, and conducted his war. Jäckel reviews Hitler's ideas on foreign policy for the attainment of his ultimate goal—the conquest of *Lebensraum*—and concludes that these ideas constituted the guidelines for the policy he actually conducted. "It was, of course, not a timetable, nor even a detailed prospectus, but a definite and structured list of objectives, priorities, and conditions" (p. 26).

Unlike his foreign policy and military decisions, Hitler's decision to initiate the Holocaust was not conveyed through a single order, and at first this policy involved considerable inconsistency, improvisation, and confusion. Jäckel is in no doubt, however, that Hitler was the prime and only instigator of the Holocaust, a conclusion he bases on three principal arguments. First, nobody else had ever advocated systematic killing by the state as a solution to the Jewish question. Second, throughout the war all participants claimed to have received their orders from Hitler. Third, given the nature of the Nazi state and its ruler, it is difficult to imagine that an act of such scope, with such far-reaching consequences for the conduct of the war, would have been initiated by subordinate agencies. With respect to Japan, Jäckel argues that Hitler's declaration of war on the United States was done to ensure Japan's entry into the war against England and to secure a Japanese commitment not to conclude a separate peace. Unfortunately, Jäckel does not deal with the curious problem of Hitler's racial views and his Japanese alliance.

Although Jäckel maintains throughout his book that all major policy decisions in the Third Reich were made by Hitler alone, he believes that this program must have accorded with the deeper tendencies and ambitions of Hitler's country and of his time. "We may not be able to explain this, and yet we have to recognize it. Was he an author or an executor, a producer or a product?" (p. 43). Jäckel confesses his inability to provide definite answers to these questions and concludes, though with obvious hesitation, that Hitler was product and executor. Hitler's master plan, he says, "was not wholly incongruent with general developments, and its realization was therefore ensured" (p. 105). Late imperial territorial conquest was presaged in the development of Germany, just as it was in the development of Japan and Italy. "Thus Hitler, notwithstanding his own great responsibility in shaping events, was no more than the executor of a longstanding tendency" (p. 105). With that, Jäckel introduces a theme stressed by a large number of his colleagues and one that he himself should have developed further, for as presently formulated it seems to contradict his most significant previous conclusions. This contradiction, however, in no way detracts from the overall value of this incisive and readable book, which should prove of great interest to all students of modern history.

NORMAN RICH
Brown University

SEPPO KUUSISTO. *Alfred Rosenberg in der nationalsozialistischen Aussenpolitik, 1933–39*. (Studia Historica, number 15.) Helsinki: Suomen Historiallinen Seura. 1984. Pp. 436.

This is a confusing book about a complicated subject. The confusion begins with the title. It is really a history of the Nazi party's *Aussenpolitische Amt* (APA), of which Alfred Rosenberg was the head but in which a number of other people played leading roles. The APA was founded in 1933, but its roots go back earlier, and its activities went beyond 1939. Seppo Kuusisto, a Finnish journalist turned historian, has done a prodigious amount of research, made difficult by missing sources. He has unearthed some new details and revised some aspects of the APA's history, but the prevailing view among historians of the APA's relative insignificance and ineffectiveness still stands. The book's chief weaknesses are poor organization and too much repetition. It could have profited from a good editor.

The book's central figure is Alfred Rosenberg, although he is often lost sight of. Rosenberg was executed as a war criminal for his deeds during the Second World War as minister for the occupied eastern territories. In the period covered here he was considered one of the Nazi big-wigs and, as the author of the turgid, unreadable, and now unread best seller *The Myth of the Twentieth Century* (1930), the official interpreter of Nazi "philosophy." He was also briefly thought to have been in line to become Hitler's foreign minister. An ideologist rather than a politician, Rosenberg believed (like Hitler) in racial

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determinism and fanatically hated Jews and Bolsheviks, who to him were synonymous. As a person he was thoughtful, loyal, modest, and not out to feather his own nest. As an intellectual he was mediocre at best—a *"Lumpenintellektual,"* self-taught and rigid (again, like Hitler) and, by all accounts, a crashing bore.

As head of the APA, Rosenberg proved to be a poor administrator and a rank amateur in the struggle for survival against his major opponents, Joseph Goebbels and Joachim von Ribbentrop. He had few supporters in higher places. Rudolf Hess sometimes intervened on behalf of the APA, and Hitler, though disappointed in Rosenberg, remained supportive as he was of most of his "old fighters." The keynote of the APA's organization and activities was improvisation. Some of its main figures were able men—Arno Schickedanz, Georg Leibbrandt, Werner Daitz, Thilo von Trotha, and others—but its program was diffuse and its work woefully underfunded. Its activities were concentrated on Europe, and they constitute the main body and contribution of Kuusisto's book. Their primary emphasis was directed toward the east, where Rosenberg hoped for the overthrow of bolshevism and the ultimate disintegration of the Soviet Union into its ethnic components. But the APA was only one of innumerable agencies concerned with foreign policy, including the Foreign Ministry, the party's foreign branch (*Auslandsorganisation*), the *Dienststelle* Ribbentrop, many organizations devoted to German minorities abroad, and Russian and Ukrainian refugee cohorts. To assert itself in this jungle of competitors, the APA needed a ruthless and charismatic leader, not Rosenberg.

This book will be useful to historians dealing with the specific aspects of German foreign policy in this period. For anyone interested in the APA's general role in Nazi foreign policy, the relevant sections of Hans-Adolf Jacobsen's *Nationalsozialistische Aussenpolitik 1933–1938* (1968) will be more accessible and rewarding.

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ERNST KLEE. *"Euthanasie" im NS-Staat: Die "Vernichtung lebensunwerten Lebens."* Frankfurt a/M.: S. Fischer. 1983. Pp. 502. DM 39.50.

Ernst Klee's book distinguishes itself from many other works about the Nazi mass killings of the handicapped and mentally ill both in its wealth of detail and in its extensive use of eyewitness accounts by participating physicians, residents of the towns housing the "killing institutions," and former psychiatric patients who managed to escape death. The author weaves this evidence together into a narrative of Nazi Germany's first foray into bureaucratic mass extermination—a foray that, according to Klee, first began not at the psychiatric institution Grafeneck in 1940, as is often assumed, but rather (on a limited scale) at select institutions several years earlier. Klee also documents far more extensively than anyone to date the history of the "Aktion 14f 13" after it was "officially" ended in November 1941, largely owing to church protests. We learn about the frightening particulars (elimination of most mentally ill, infirm, and politically undesirable concentration-camp prisoners), the continuation of "child-euthanasia" through lethal injections and slow starvation, and the further elimination of Jewish, Polish, and Russian patients.

Despite its richness of detail and its fruitful exploitation of new source material, this book remains largely a chronicle of events lacking any analysis. This is certainly in large part intentional. Klee informs us in his preface that he wishes to portray the history of the *Aktion* "without commentary." As a journalist with training in the education of the handicapped, his intention is perhaps understandable, for an important motive for writing this work is to call attention to the maltreatment of handicapped people. Yet, insofar as he merely presents the "facts" and attempts to engage his reader on the strength of the numerous accounts and testimonials, Klee is not really writing history. He often alludes to pertinent information that, had it been analyzed and interpreted, could have placed Nazi euthanasia in a more sophisticated social and intellectual context. For example, when he discusses the background to these mass murders in his first chapter, he mentions a few figures in the German eugenics movement but fails to analyze the connection between eugenics and euthanasia. Certainly, the topic warrants more than the few pages devoted to it by Klee. A more serious shortcoming is his treatment of the social and professional background of the "euthanasia personnel." The author tells us that they all came from middle-class backgrounds. But what does that mean? It is necessary to show how such a background shapes the attitude of those involved in ways that can explain why they would participate in mass murder. A critical discussion of the hereditarian tradition in German psychiatry, for example, would help explain some of the disdain toward many classes of "mental defectives" on the part of some medical professionals, but it is absent from the text.

The considerable limitations of the book notwithstanding, Klee's work has made a useful contribution to the growing literature on medicine under the swastika. His implicit thesis that euthanasia was a conscious policy to rid the Reich of individuals who were seen as a financial or social liability fits in well with the recent emphasis in the field on productivity as a leitmotif of Nazi medicine. Yet even here the thesis is only implicit. The important task of assess-

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ing why a significant portion of the medical community was willing to support, or at least tolerate, the wholesale destruction of "lives not worth living" still remains to be tackled.

SHEILA FAITH WEISS
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GESCHICHTSVEREIN DER DIÖZESE ROTTENBURG-STUTTGART, editor. *Kirche im Nationalsozialismus.* Sigmaringen: Jan Thorbecke. 1984. Pp. 299. DM 24.

BERNHARD HANSSLER. *Bischof Joannes Baptista Sproll: Der Fall und seine Lehren.* Sigmaringen: Jan Thorbecke. 1984. Pp. 140. DM 18.

Since 1945 the historiography of the German Catholic church during the Nazi era has gone through three stages: first, a period of glorification of the heroic resistance against Nazi persecution, which enabled the church to survive almost intact; second, a more critical evaluation in the 1960s that asked searching questions about the church's readiness to collaborate with the Nazis in many fields and about its failure to take any significant steps to assist other victims of Nazi oppression, particularly the Jews; and, third, a more balanced academic approach, which, though still very defensive in tone, strives to reconstruct the Catholic milieu and the atmosphere of a minority community faced with unprecedented dangers.

On the occasion of the fiftieth anniversary of the Nazi takeover of power, a group of academics and surviving participants from the south German diocese of Rottenburg-Stuttgart in Württemberg convened a conference whose report is now published. Together with useful eyewitness recollections of the church's life in this corner of the Reich, several scholars give serviceable accounts of the present state of the discussions. One senior priest of the diocese contributes his experiences in the small town of Ellwangen; another reprints three contemporary sermons together with notes deciphering the encoded anti-Nazi formulations contained therein.

The more general surveys of Catholic life in the Nazi era show that the younger scholars are less prepared than their elder colleagues to continue the apologetic tone of believing that the church took the best possible line of self-defense against the Nazi terror by seeking to reconcile its national with its ecclesiastical loyalties. Karl-Heinz Wiest's essay on the impact of Rolf Hochhuth's play *The Deputy* presents an excellent summary of the issues involved in the church's silence over the fate of the Jews. Hans Kreidler evaluates critically the influence of Karl Adam, then the star professor of Catholic theology at Tübingen University. Adam, like his Protestant counterpart Paul Althaus at Erlangen, enthusiastically endorsed the new Nazi regime, believing that the dynamic idealism of the Nazis could be combined with Catholic loyalties in a new revival of the German blood. Unfortunately, appropriate disciplinary measures by the Vatican were never instituted. Useful articles are also included on such topics as the Catholic trade unions, the Catholic lay organizations, and the Catholic press.

The value of such conferences lies in collecting, while there is still opportunity, the reminiscences of participants in the church struggle and combining them with broader analyses. Unfortunately, this particular volume still reflects the unchanged perspectives of the past, being written by Catholics about Catholics for Catholics. Future local church histories would be greatly enriched by an ecumenical approach, so that the equally noteworthy experiences of other churchmen could provide a comparative evaluation of how the Nazis persecuted all the churches in each locality. Württemberg would have been an excellent place to start.

One of the contributors, Bernhard Hanssler, has also compiled a popular memoir of J. B. Sproll, then the Catholic bishop of the area, that, however, is of little help to the historian. Sproll was the only bishop known to have refused to vote in the Nazi-organized plebiscite in April 1938 to approve the annexation of Austria. He was subsequently the victim of rowdy demonstrations by Nazi thugs, organized by the Nazi *Gauleiter* Wilhelm Murr, and was finally banned from his diocese—the only Catholic bishop to be so humiliated. But Hanssler's pious hagiography does not explain why the bishop decided on this particular personal protest or why he was virtually abandoned by his fellow bishops and even by the Vatican. Nor does it clarify the Nazis' motives. Was this intended to be a prelude to similar evictions of known opponents of the regime or merely overexcessive vindictiveness by a particular, violent *Gauleiter* on this local level? Instead, Hanssler contributes a handful of anecdotes and a lengthy chapter on his own running battles with the Nazi authorities. In his view the careful research of later historians has uncovered many facts but fails to capture the atmosphere of the time. "An hour's interrogation by the Gestapo would be an excellent qualification for all historians of Nazism," he claims (p. 10), but, despite being himself so qualified, he fails to provide any insightful analysis of the particular circumstances of his hero and patron. The historian should refer to the useful collection of documents, edited by Paul Kopf and Max Miller, *Die Vertreibung von Bischof Joannes Baptista Sproll von Rottenburg 1938–1945* (1972).

JOHN S. CONWAY
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FRIEDRICH PRINZ, editor. *Trümmerzeit in München: Kultur und Gesellschaft einer deutschen Grossstadt im*

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Aufbruch, 1945–1949. Munich: C. H. Beck. 1984. Pp. 459. DM 48.

This large-format, double-columned, heavily illustrated volume was produced to accompany an exhibition (carrying the same name as the book) put on in the Munich city museum. It is not, however, a catalogue but rather a collection of four reminiscences and thirty-five essays by thirty-one different authors on a variety of themes pertaining to cultural and social life in that city in the first postwar years. The essays are grouped in seven sections: politics and culture, historic preservation and pictorial arts, churches and education, music and theater, literature and the media, a literary retrospective, and a "Kaleidoscope of Daily Life amidst the Ruins." "High culture" gets most of the attention, but the last section, with essays on such topics as fashions in clothing, the struggle to find food, and the first fall carnival after the war, is representative of the growing interest of German historians in the everyday life of average people.

Does this work merit the attention of historians? The answer is yes. No information is given about the authors of the essays, but I know that many were students in Friedrich Prinz's historical seminar at the University of Munich, and the orientation of most of the essays is serious and scholarly, reflecting extensive research in the archives and published literature. The essays are well documented. Moreover, a valuable glossary at the end contains capsule biographies of the key figures in the city and descriptions of important institutions and organizations. There is also a cumulative bibliography of unpublished and published sources. Hence, this will become an extremely useful reference work for scholars interested in the modern history of Munich.

Prinz does not claim that the essays constitute a definitive history. He acknowledges major gaps, such as the absence of treatment of the Technical University and the role played by state politics. (Since Munich is the capital of Bavaria, the latter is a serious omission.) The essays do, however, challenge some stereotyped views of the postwar era. The role of American cold war ideology was not always decisive in shaping cultural policy. Although there were some real breaks with the past, as for example with Radio Munich, a great deal of continuity was to be found in the form of traditional cultural taste and values. One of the most interesting essays, that by Nina Krieg on historic preservation and the physical reconstruction of the city, documents the hold of Munich's classicist architectural tradition on postwar planners. Although not a comprehensive city history, this book should, as Prinz hopes, serve as a starting point for future research on modern Munich, and it will also serve as an example for those studying the revitalization of other cities destroyed in the war.

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LEWIS LOCKWOOD. *Music in Renaissance Ferrara, 1400–1505: The Creation of a Musical Center in the Fifteenth Century*. (Studies in the History of Music, number 2.) Cambridge: Harvard University Press. 1984. Pp. xxii, 355. $35.00.

During the course of the fifteenth century, Ferrara emerged as a major center of musical culture in Renaissance Italy, far surpassing the other small princely courts such as Mantua and rivaling the achievements of Florence, Milan, and the papal court. This ambitious study by Lewis Lockwood, a noted musicologist, aims at a comprehensive historical explanation of Ferrara's musical development. No scholar is in a better position to do so. This book reflects the author's more than fifteen years of probing in the immense archival sources for the Ferrarese court preserved in the Archivio di Stato in Modena. Lockwood's mastery of unpublished sources has permitted him to provide authoritative answers to such questions as how the various musical institutions in Ferrara were organized and staffed, where the musicians came from, how they were recruited and paid, in what settings and for what purposes they performed, and what their status was compared with other figures at court.

His book also examines in detail the musical procedures and styles of compositions, both sacred and secular, produced for the Estensi. The new evidence Lockwood adduces and the new interpretations he advances doubtless will stimulate debate among Renaissance musicologists for some time to come. What makes his book especially noteworthy for cultural and intellectual historians of the Italian Renaissance, however, is the larger and more difficult issue he addresses: the connection between the artistic context of Ferrara and the characteristics of the music created there.

In this regard, Lockwood sees two major forces as active in Ferrara throughout the fifteenth century. One was the Ferrarese state itself in its broader political and cultural meaning—that is, in the city's communal consciousness and its civic traditions, which found expression in both religious ceremony and civic festival. The second was the ruling family, the longest-established and most continuous princely dynasty of Renaissance Italy. Estensi ties to the French and Burgundian courts and to Naples provided impetus for general cultural developments, such as the attractiveness of French chivalric mores, and for specific developments, such as the opportunities to make or maintain ties with Franco-

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Burgundian composers, among them Guillaume Dufay and, later, Johannes Martini.

But important shifts occurred as well. The different aesthetic tastes and religious sensibilities of individual Estensi rulers influenced their musical patronage. Lockwood is particularly attentive to these differences, and his characterizations of Leonello (1441–50), Borso (1450–71), and Ercole (1471–1505) seem sensitive and astute. An even more fundamental force for change, Lockwood suggests, was the rise of humanism with its attendant intellectual and aesthetic commitments. The rhetorical values of humanism encouraged polyphonic composers (particularly Josquin Desprez in the music he wrote for Ercole) to respond more expressively to the demands of text. This in turn entailed a new mode of musical perception.

Lockwood's stress on indigenous Italian factors in Ferrara's musical development may well occasion controversy. His analysis, however, helps relate the history of Italian Renaissance music more closely to the general cultural and intellectual trends of the period.

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EMILIA MORELLI. *Mazzini: Quasi una biografia.* (Risorgimento Idee e Realtà, new series, number 5.) Rome: Edizioni Dell'Ateneo, for the Istituto per la Storia del Risorgimento Italiano, Rome. 1984. Pp. xii, 278.

In this book Emilia Morelli, the *doyenne* of Italian Mazzini scholars, has collected sixteen essays, published from 1960 to 1980 in various journals, Festschriften, and proceedings. Although written at different times and for different audiences, these essays have a cohesiveness and unity that justify the subtitle, "Almost a Biography." Mazzini does not lend himself easily to a biographical approach. From youth his life was synonymous with political action. In contrast to other nineteenth-century revolutionaries like Marx, Herzen, and Bakunin, Mazzini did not allow himself the distractions of domesticity. Thus, any biography of Mazzini must perforce also be a history of the Risorgimento. Morelli succeeds in telling us much about both.

Since the essays were written as individual pieces, each may be read without reference to the others. Read sequentially, however, they are indeed "almost a biography." The first three outline Mazzini's life and deal with his republicanism and Italian nationalism, the twin principles of his political creed. The next four concentrate on Mazzini's activities in the last two decades of his life, when support for him dwindled and he struggled to hold his followers together. Focusing on different moments in Mazzini's activity after 1849, these essays survey in particular the years 1849–59, 1860, and 1861 and his various plots to liberate Venetia from 1861 to 1866. Turning to Mazzini's views on non-Italian issues, Morelli analyzes his articles on English politics, written for French and Swiss journals shortly after his arrival in London in 1837. Mazzini quickly grasped the essence of English political life but was never able to reconcile his messianism with the utilitarian outlook of Victorian England. Not so successful is the discussion of Mazzini's hostility to the Paris Commune, a position that brought him into conflict with the European Left and embittered his last two years. Two other essays treat Mazzini's concern for the Italian street children stranded in London, a concern shared and supported by English humanitarians, and Metternich's efforts to infiltrate Mazzini's conspiracies in 1843–44. In conclusion, Morelli explores how Francesco De Sanctis, the great nineteenth-century literary critic, and the twentieth-century historians Walter Maturi and Adolfo Omodeo have interpreted Mazzini.

Each essay reveals Morelli's profound knowledge of Mazzini's writings and of the secondary literature. Together, they provide an indispensable source for an understanding of Mazzini, a man too frequently misconstrued by contemporaries, like Carlo Cattaneo and Giuseppe Garibaldi, or misinterpreted by later scholars, like Giovanni Gentile. If anyone can write a biography of Mazzini, it is Morelli. One hopes that, having given us "almost a biography," she will now write the definitive biography.

EMILIANA P. NOETHER
University of Connecticut

VINCENZO G. PACIFICI. *Francesco Crispi, 1861–1867: Il problema del consenso allo Stato liberale.* (Risorgimento Idee e Realtà, new series, number 6.) Rome: Edizioni dell'Ateneo, for the Istituto per la Storia del Risorgimento Italiano, Rome. 1984. Pp. 311.

Most historians of modern Europe associate Francesco Crispi with his autocratic governments of the 1890s that repressed the *Fasci siciliani* and led Italy to its colonial disgrace at Adowa. The present volume is concerned, instead, with Crispi's far less familiar democratic activity during the parliamentary deliberations that established the institutions of the newly united kingdom and anguished over the most efficacious means of acquiring Rome and Venice. As such, this work is an account of Crispi's evolution from Garibaldian revolutionary and Mazzinian republican to collaborator in the extension of the Piedmontese monarchy's institutions throughout the peninsula. Thus, it chronicles Crispi's arrival at the view that the House of Savoy

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was essential to the preservation of national unity. As he said in 1864, "The monarchy united us; the republic would divide us."

Vincenzo G. Pacifici limits his study to Crispi's election campaign, his entry into the first parliament of united Italy in 1861, and his role in the debate that followed Garibaldi's ill-fated attempt to conquer Rome, which terminated in the Mentana defeat of 1867. The account is divided into five chapters that center on parliamentary activity and bring Crispi in as a major protagonist throughout the period covered. The rather tiresome account of parliamentary exchanges and concurrent journalistic comments provide the context for placing Crispi within the whole and illustrate his rationale for accepting a monarchy that he sought to inspire with liberal views. Nonetheless, Crispi often fails to stand out among his colleagues to the extent one would expect from the book's title, let alone from the chapter titles.

In the fourth and fifth chapters the author appears to find an interpretive thesis when he makes clear that Crispi reveled in being a deputy and that his actions can be divided between those of a *pars destruens*, as when Crispi cast himself as an opponent of the majority, and a *pars construens*, as when he displayed deep commitment to law as the basis of conduct for men and for management of the state. From these examples, however, one can see that Pacifici's judgments are somewhat superficial. Even so, this glimmer of synthesis provides welcome relief from the tiresome and often confusing adherence to a strictly chronological presentation that easily loses the development of issues over the years.

Although his presentation is plodding and pedantic, Pacifici's research is thorough and methodical. He has made full use of archival material as well as of the contemporary press, memoirs, and more recent monographs. The result is a solid contribution to our knowledge of Crispi, and one that contrasts with the picture of him as the more famous and controversial prime minister at the end of the century. Another phase in this Sicilian statesman's career also deserves equally solid treatment: the middle years of his parliamentary life, with such accomplishments as the administrative reforms of the late 1880s. When Crispi's whole career is reviewed, it may be possible to conclude, as Pacifici does for the years 1861–67, that one finds neither a hero nor a devil.

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SANDRO ROGARI. *Ruralismo e anti-industrialismo di fine secolo: Neofisiocrazia e movimento cooperativo cattolico.* (Quaderni di Storia, Sezione Documenti, number 67/10.) Florence: Felice Le Monnier. 1984. Pp. 272. L. 20,000.

Scholarly interest in the processes of modernization, as Arno Mayer has recently argued, has resulted in the neglect of those forces of tradition and continuity that persisted into the modern era. Such neglect indeed seems evident in the recent works on Italian fascism. In the view of Sandro Rogari, these studies have stressed the role of fascism in the processes of industrial concentration at the expense of the regime's traditionalist, paternalistic aspects. As a result, too little attention has been given to the ways fascism achieved an important base of consensus, especially among Catholics, by revitalizing older rural myths and themes. Accordingly, Rogari proposes to explore the historical roots of *ruralismo* in the prewar era and thereby to contribute to our understanding of the nature and characteristics of the rural dimension in Italian popular culture.

The high hopes raised by Rogari's insightful introductory comments are unfortunately not fulfilled in the body of the book. He provides essentially a conventional history of ideas with scant connection to either popular culture or more general developments in Italian society. The first part of the study is devoted to a detailed explication of the ideas of the agronomist Stansilao Solari and his small group of neophysiocratic disciples in Parma between the 1870s and the First World War. An active and innovative farmer himself, Solari in his writings repudiated industrialism and the life of the city. His alternative was an agricultural *risorgimento* based on newer developments in agronomic science that promised to increase dramatically the fertility of the soil. Solari envisioned a "third Italy," a rural nation that would be an organic community, free of conflicts and governed by enlightened landowners who once again resided in the countryside.

The second part of the book provides an equally detailed account of Solari's wider influences and the debates and polemics engendered by his ideas. Rogari is especially concerned to show how some of Solari's ideas touched an audience beyond traditionalist circles, including radicals from Parma as well as the great liberal economist Luigi Einaudi. But, above all, the author emphasizes the close links between Solari's group and the Catholic cooperative movement.

Two sorts of problems beset this thorough but narrowly researched book. By offering a strictly chronological treatment of Solari's writings rather than a synthesis of the main themes, Rogari's presentation tends to be annoyingly repetitive. More seriously, much of the evidence included in the text seems to diminish both the extent and depth of Solari's influence. The agronomist's intransigent opposition to protectionism and agrarian interest-group politics limited his influence in the larger agricultural community, while his strong preference for the paternalistic large landowner over the small

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peasant proprietor soon led to a rupture with the Catholic cooperative movement. Even when his agronomic ideas were embraced, they were stripped of their anti-industrial and ruralist implications. In the final analysis, the author's methodology and research design simply do not allow him to explore the important historical issues and questions that he raises in his preface.

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BRUNELLO VIGEZZI, editor. *Federico Chabod e la "Nuova Storiografia" italiana dal primo al secondo dopoguerra, 1919–1950.* (Edizioni Universitarie Jaca, number 4.) Milan: Jaca. 1983. Pp. xxiii, 719. L. 38,000.

Federico Chabod (1901–60) was one of Italy's most eminent historians. Born in Val d'Aosta, he studied under Pietro Egidi at the University of Turin. He undertook advanced studies in Friedrich Meinecke's Berlin seminar and Gioacchino Volpe's Scuola di storia moderna e contemporanea in Rome, where he was one of the "four musketeers" (the others being Nello Rosselli, Walter Maturi, and Carlo Morandi), who held three-year research appointments between 1930 and 1934. In those years Chabod contributed several important articles to the new *Enciclopedia Italian Treccani,* and in due course he won professorships at the Universities of Milan and Rome. In 1943–45 Chabod was active in the Resistance in the Val d'Aosta. After the war he taught at the University of Rome and directed Benedetto Croce's new Istituto per gli studi storici in Naples. In 1955 Chabod was elected to the presidency of the bureau of the Comité international des sciences historiques, but his untimely death from cancer prevented him from presiding at its tenth congress in 1960.

Chabod's historical interests were wide-ranging. Although he sometimes did archival research on specific problems (for example, at Simancas on Charles V and the Duchy of Milan), Chabod preferred analyzing broad historical periods. His publications include essays and monographs on the Renaissance, Machiavelli, Charles V and the Duchy of Milan, Giovanni Botero, the Counter Reformation, the Enlightenment, and the bourgeoisie. Chabod's masterly *Storia della politica estera italiana dal 1870 al 1896* (1951) was his greatest work. His approach to the history of foreign policy was much more sophisticated than that of traditional diplomatic historians. In the first half of that seven-hundred-page book, Chabod analyzed the "passions and ideas" (the keystone being the "idea of Rome") that were the premises of Italian foreign policy; in the second half he discussed individuals who shaped Italian foreign policy between 1870 and 1896.

Twenty years after Chabod's death, scholars at the University of Milan decided to plan a conference on Chabod's impact on Italian historiography and on the restructuring of professional organizations for historical study. Coordinated by Brunello Vigezzi, who also edited this verbatim record of the proceedings, the conference took place in March 1983. A dozen major themes were explored, each introduced by a specialist. Ernesto Sestan, a contemporary of Chabod, delivered the prolusion, "Federico Chabod and the 'New Historiography': Profile of a Generation of Historians."

The other topics were "Studies of Medieval History" written by Girolamo Arnaldi, "The Problem of the Renaissance" by Sergio Bertelli, "Regional History and the Formation of the Modern State" by Giuseppe Galasso, "Reflections on Chabod's 'Per una storia religiosa dello Stato di Milano'" by Giorgio Spini, "Historical Studies of Political Theories and of the History of Ideas" by Gennaro Sasso, "The Problem of Historicism" by Fulvio Tessitore, "Economic History in the Era of Federico Chabod" by Luigi De Rosa, "The 'New Historiography' and the History of International Relations" by Brunello Vigezzi, "The Organization of Historical Studies" by Armando Saitta, "The Contribution of Italian Historiography to the International Congresses of the Historical Sciences during the First Half of the Twentieth Century" by Karl Dietrich Erdmann, "Fascism and the Historians" by Renzo De Felice, and "The 'New Historiography' between Political Engagement and Scientific Research: Stages and Problems, 1945–1950" by Furio Diaz.

All of the papers and discussions will be of value to specialists. The reports by Sestan, Vigezzi, Diaz, and De Felice are of particular interest to this reviewer. De Felice probes the question of whether Chabod and his contemporaries in Volpe's school accommodated themselves to the fascist regime. (Volpe himself was a convinced fascist.) On the basis of De Felice's investigation and the ensuing discussion, it seems clear that, of the "four musketeers," Morandi was the most sympathetic toward fascism, whereas Rosselli was the most hostile, followed by Maturi and then Chabod. To aspire to any professional advancement, Chabod had to join the Fascist party in 1932, but, apart from that, he avoided compromising himself. De Felice also concludes that Chabod chose his research topics independent from politics and that his historical philosophy was shaped more by Volpe's brand of socioeconomic history than by Croce's historicism.

As one might expect, a number of questions remain unanswered. Everyone agreed that Chabod had compassionate understanding of the dilemmas confronting many historical personages and that his

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wide-ranging interests included not only sociopolitical and diplomatic history but also the Crocean history of ideas. But he had little interest in the history of science, historical demography, or quantitative history. Whether a truly "new" historiography emerged in Italy between 1919 and 1950 is debatable. Perhaps a more plausible argument can be made that what was "new" emerged largely after World War II, under the impact of Marxism, the *Annales* school, and other influences.

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MICHEL OSTENC. *L'éducation en Italie pendant le fascisme*. (International Series, number 12.) Paris: Sorbonne. 1980. Pp. 422.

This book encompasses three themes of Italian fascist historiography: the role of Giovanni Gentile, Italy's educational system under Mussolini, and fascist youth culture.

Gentile, Italy's most eminent philosopher after Benedetto Croce, served as Mussolini's first minister of education. He was not the only intellectual of the idealist school to see in fascism an expression of his own sentiments, a healthy mix of thought and action. In Gentile's case, his Hegelian concept of an absolute albeit ethical state, "the synthesis of individual consciences," facilitated acceptance of the fascist dictatorship. Yet, while deploring Gentile's self-deception, Michel Ostenc justly observes the divide between his philosophy and fascism. Whereas Gentile wished to fulfill the Risorgimento, fascism was its contradiction. Even within Gentile's authoritarian state there was room for *liberté d'esprit*, clearly irreconcilable with Mussolini's ambition to create the new fascist man. Thus, "the convergence of idealism and Fascism was fortuitous" and Gentile's appointment to office "the fruit of Mussolini's opportunism and political skill" (pp. 37–38). In the same vein Ostenc refutes the contemporary boast that Gentile's modification of Italy's educational structure in 1923 was "the most Fascist of reforms."

Gentile concentrated his efforts on secondary and university education, leaving reform of primary schooling to his lieutenant, Giuseppe Lombardo-Radice. By drawing on pamphlet and periodical literature of the day, Ostenc captures both the spirit and substance of the lively pedagogical debate behind the 1923 measures. These accentuated classical and humanistic studies in curriculum and discipline, competition, and elitism in instructional method. Administrative centralization and national state examinations were juxtaposed with a degree of classroom autonomy, which unintentionally was to provide a modest opportunity for some antifascist teaching. The Catholic religion was given official status in primary schools, not to promote faith but to inject a moral element into education. Ironically, Catholicism, after the concordat of 1929, would prove a potent foe of Gentile's secular idealism in education.

Gentile's heyday, in fact, was brief. He was dismissed in the post-Matteotti wave of fascistization. In 1927 the fascist Grand Council paid lip service to his 1923 reform but recommended, "under pretext of strengthening it, measures to denature it" (p. 178). This meant fascist textbooks and loyalty oaths for teachers. The nadir of fascist *caporalisation*, however, came in 1935 with the arrival at the education ministry of Cesare De Vecchi, who wore his black shirt in the Palazzo Minerva and required his bureaucrats to stand at attention in his office with arm outstretched in the fascist salute. His replacement by the "enlightened" fascist Giuseppe Bottai was too little, too late to salvage much of Gentile's legacy.

Because of Gentile's eclipse, Ostenc's pages on youth culture in the 1930s deal less with formal education than with fascist indoctrination. The lessons in militarism and nationalism were intended primarily for the young elite, the future leaders of the regime. Yet it was precisely among the more intelligent secondary school and university students that party propaganda was least successful. Criticism voiced at the annual *Littoriali* betokened a youthful "fronde," which sprang into the open during World War II. No new interpretative windows are opened here. The book's originality lies in its earlier linkage of pedagogical ideas and fascist reality.

It is typically French scholarship. Although thorough in his own way, Ostenc largely eschews English-language authorities, and some important titles published in the 1970s (notably on Bottai) are conspicuous by their absence. Notes are mercifully placed at the foot of the page, but the bibliography is inconveniently arranged, and there is no index.

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JAN KARSKI. *The Great Powers and Poland, 1919–1945: From Versailles to Yalta*. New York: University Press of America. 1985. Pp. xvi, 697. Cloth $39.75, paper $28.50.

This book, the fruit of twenty years' research, covers the interwar period and World War II; the section on the latter, which is clearly Jan Karski's main interest, is the better of the two. Although he claims that the book has no message or judgments, he clearly believes that the Western powers betrayed Poland in both 1939 and 1945. Some judgments are more controversial than others—especially because Karski relies heavily on memoirs and only sparsely

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cites archives and recent British and American monographs.

Karski's knowledge of the interwar period is not extensive and some judgments are dubious. To anyone familiar with French diplomatic and military policies, the French military's view of Poland as unstable was clearly not the main cause for their lack of plans for joint military action (p. 108). Lack of British support, the trauma of World War I, and the resulting defensive psychology and strategy led the French to see both Poland and Czechoslovakia as liabilities after 1925. Likewise, Britain did not intend to defend the status quo of Eastern Europe in 1939.

The author's weak grasp of British and French policy may be partly responsible for his judgments on Polish Foreign Minister Jozef Beck, whose policy in the Rhineland crisis was not as ambiguous as Karski makes out. Most Poles condemn Beck's policy toward Czechoslovakia in 1938, but most, like Karski, also forget that both the Polish opposition parties and public opinion demanded the return of Teschen to Poland. The Polish ultimatum to Lithuania in March 1938 was not designed to show off Polish strength and focus international attention on Warsaw (p. 207). The Poles had made numerous fruitless attempts to normalize relations with that country, and the ultimatum was sent to prevent German domination of Lithuania. Beck did not believe that the Polish-German agreement of 1934 was a permanent fixture, but he did believe, as did most European statesmen, that Hitler was not mad enough to risk another world war. The Polish government showed its expectation of war, however, by developing the Central Industrial District. In view of the known appeasement policy of France and Britain, how could Beck have worked to strengthen the Versailles system and the League of Nations? In spite of his critical treatment of Beck in the book, Karski admits that no other Polish foreign minister could have changed the course of events (p. 324).

Great power policy toward Poland in World War II is ably presented but suffers from an inadequate grasp of Roosevelt's thinking and policy. The old view that Roosevelt "trusted" Stalin was debunked by Robert Dallek in 1979. Roosevelt told Adolf C. Berle as early as April 30, 1942, that he did not personally mind Stalin taking all the territories he claimed. Roosevelt's concern was not to alienate U.S. public opinion and especially Polish-American voters. As for Churchill, he was almost equally concerned with British opinion, as well as with the Polish army in the West. Karski's grasp of the activities of Polish Communists in occupied Poland and the USSR is also weak. B. Bierut went to the USSR in 1939, not 1935. The Polish Committee of National Liberation was composed not just of members of the Union of Polish Patriots but also of members of the bogus Polish National Council. Finally, the Red Army had no Polish detachments (p. 530); General Z. Berling led a Polish army under overall Soviet command.

In sum, the reader will find Karski's account of great power policy toward Poland in World War II more reliable than his treatment of the interwar period. Although Karski's study is a good guide to World War II, it leaves much to be desired. Finally, any definitive study of the topic must use the mass of Polish government documents available, though not yet catalogued, in the Hoover Archives.

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LÉON NOËL. *La Pologne entre deux mondes.* Foreword by J.-B. DUROSELLE. (Internationale, number 23; Collection Historique de l'Institut d'Etudes Slaves, number 24.) Paris: Publications de la Sorbonne or Institute d'Etudes Slaves, Paris. 1984. Pp. 288. 130 fr.

Almost forty years after the appearance of the memoirs of his dramatic embassy in Warsaw (1935–39), Léon Noël has again turned his attention to interwar Poland. The result is a collection of "tableaux et portraits" written with verve and bite in which perception and prejudice are recorded with style and passion.

The general line of Noël's criticism of Polish foreign policy has remained consistent over the years: Warsaw was deluded by the possibility of continued good relations with Germany; this decision in turn caused the Poles to place greater emphasis on relations with Berlin than on those with London and especially Paris, to say nothing of Prague and Moscow (pp. 26–27, 138–39, 268). The chief architect of this doomed policy was Colonel Józef Beck, the bête noire of Noël's reminiscences. Indeed, the criticism of Beck and of a number of other prominent personalities is repeated and striking, frequently vicious and *ad hominem*. The reader is left with the impression that Noël is less critical of the content of Polish foreign policy than of its architects and style.

Beyond his criticism of Beck, Noël does not attempt to provide a systematic analysis of Polish foreign policy. Indeed, he quotes without contradiction Piłsudski's remark about Poland being unable to rely on France (p. 95), admits that Anglo-French inertia was at least partly responsible for Warsaw's "tilt" toward Berlin (pp. 27 n.19, 142), and specifically rejects the charge that Beck or anyone else in the regime was pro-Nazi or pro-German (pp. 104–15, 195). By contrast, he excoriates French policy for its ineptitude and disastrous lack of direction and initiative (pp. 30, 218, 230–51). In effect, he presents a good case for Poland's estrangement

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from Paris (pp. 32, 214). Hence, there is considerable irony in Noël's repeated criticism of various Poles for their lack of "friendship" for France, which seems to have been the litmus test for correct statesmanship. Significantly, Noël's admitted involvement in a 1936 effort to oust Beck (in conjunction with the Rambouillet negotiations) was designed to replace the colonel with someone less independent and more amenable to French influence (pp. 117–20, 128).

Students of Franco-Polish relations will discover only a few passages that add substantively to Noël's *L'agression allemande contre la Pologne* (1946). The discussion of Marcel Boussac and the "affaire de Żyrardów" sheds some intriguing light on the role of French capital in Poland. Notable too is the analysis of the Polish aspect of the Anglo-French approaches to the Soviets in the summer of 1939 (pp. 225–32, 246–52). Noël sprinkles his work with a few interesting remarks and extracts from notes and documents especially about Polish-Czech relations in 1938.

Parts of this volume were written more than twenty years ago, others quite recently. Essentially chronological chapters discussing the events of Noël's tenure in Warsaw are interspersed with engaging and often perceptive essays on various aspects of Poland in the 1930s: the minorities, the peasantry, an anachronistic aristocracy, and "le caractère polonais," among others. Personality portraits abound, many of which are etched in acid. In sum, this is a gracefully written account of Poland and Franco-Polish relations in which personality dominates over policy and old antipathies have refused to mellow. Leaving aside the traces of rancor and musing over Masonic plots and similar strange elements, we are left with vivid sketches by an acute, sympathetic, but not disinterested observer.

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NEIL ORPEN. *Airlift to Warsaw: The Rising of 1944*. Norman: University of Oklahoma Press. 1984. Pp. vii, 184. $14.95.

On August 1, 1944, as Soviet armies approaching the Vistula River seemed likely to be driving into Warsaw within a day or two, the *Armia Krajowa*, the underground army of the Polish exile government in London, seized control of the city. While the Germans rushed in SS troops, the Russians stopped just across the river to await the outcome, which they thereby made inevitable. On August 12, the Soviet government having by then refused to give any kind of help to the insurgents, Prime Minister Winston Churchill ordered British, South African, and Polish emigre elements of the Allied Balkan Air Force stationed in the Brindisi-Foggia area of southern Italy to mount a supply airlift to Warsaw. The distance to be flown, over eight hundred miles one way, was close to the extreme range limit of the Balkan Air Force's B-24 Liberator and Halifax bombers, and the flight path crossed German-occupied Yugoslavia, Hungary, Czechoslovakia, and southern and central Poland. Of 186 single aircraft sorties flown in August and September, less than half reached their destination, and the loss rate, 16.8 percent, slightly exceeded that of the U.S. Army Air Force's Schweinfurt-Regensburg raid, 16.6 percent, which is generally considered to have been the most costly in the war.

The ground action has been the subject of a number of Polish and German accounts. Neil Orpen, a retired South African Army colonel who has written extensively on that country's military history, is the first to relate in more than summary fashion what went on in the air. From pilots' logbooks and interviews with former crewmen of No. 2 Wing, South African Air Force, he has reconstructed a number of flights from takeoff to landing, including several in which the landings were made impromptu, far from the home base. These may well be the best descriptions of night bomber flying in the entire World War II literature.

Nevertheless, it is too bad that Orpen did not research the larger aspects of the airlift more thoroughly. At this late date, he ought certainly to have managed a less equivocal assessment of the American role. Reluctance to test the seemingly petulant Soviet August 15 "objection" to American or British aircraft engaged in supply flights to Warsaw landing on Soviet territory did not determine the American response. The United States ambassador in Moscow had by then already requested and been categorically refused permission for landings on Soviet airfields. (Since American bombers operating by day had to have fighter escorts, they could not make round trips from England or Italy.) The American mission flown on September 18 was "too little and too late" because the Soviet government persisted in its refusal to extend landing rights until then, not because of foot-dragging in Washington.

Similarly, a deeper look into British war policy could have modified the impression that Churchill's order of August 12 was a purely altruistic gesture toward an ally who had committed a horrendous blunder. To dissociate the uprising from British strategy diminishes the sacrifice of the men who flew to Warsaw and their role in an event whose implications the Polish people will not soon forget.

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ROBERT A. KANN and ZDENĚK V. DAVID. *The Peoples of the Eastern Habsburg Lands, 1526–1918*. (History of East Central Europe, number 6.) Seattle: University of Washington Press. 1984. Pp. xvi, 543. $30.00.

Robert A. Kann and Zdeněk V. David begin this cooperative work (genuinely coauthored, for much of the revision was completed before Kann's death) by asking what is meant by the term "eastern Habsburg lands" and whether in any real sense there was a bond among these lands during the period 1526–1918. The authors quickly and easily define the term in an ethnic and historical sense as including all but the Austro-German and Italian populations of the empire. More laboriously, they work toward the conclusion that diversity characterized the eastern lands and that only through the west's belated recognition of and awakening to the culture of those lands did their populations sense any common bond.

Because the second definition, though undoubtedly correct, is essentially negative, the work as a whole lacks unity. Kann and David maintain coherence by imposing an outline of political evolution: government by the estates (1526–1620), royal absolutism and administration by the estates (1620–1740), royal absolutism and bureaucratic administration (1740–1847), and the constitutional era (1848–1918). The authors examine each of the eastern Habsburg peoples separately within these phases, including a brief description of the economic conditions in each of the lands. In the penultimate chapter they trace the evolution of cultural life, defining it in terms of response to broader European movements but again dealing with each of the peoples separately.

One could quibble with this or that particular in the political narrative, but the authors do provide, if one follows each of the nationalities, a series of concise and valuable introductions to the various sections of the eastern Habsburg empire. The chapters on the periods 1740–1847 and 1848–1918 come closest to providing interrelated narratives in which the reader remains conscious of the broader context of the other eastern lands and of the empire itself. One is tempted to ascribe this to the force of the events themselves, but the style here seems more compelling. Whether that is a clue to the limits of coauthorship or merely coincidental is not clear.

The most disappointing chapter is the one on cultural life. Aside from the difficulty (if not impossibility) of the task—to write a thirty-page summary of over three hundred years of cultural history when one assumes no common culture—the premise that cultural development occurred chronologically parallel to political development is fundamentally flawed. The result is a laundry list, or rather lists, which will be of little value to readers at any level except as a glossary.

As one volume in a multivolume series on East Central Europe, this work does fulfill an important purpose and will serve as a useful reference. It is not a monograph to be read through. The volume concludes with a brief, but very helpful, bibliographical essay.

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HORST HASELSTEINER. *Joseph II. und die Komitate Ungarns: Herrscherrecht und ständischer Konstitutionalismus*. (Veröffentlichungen des Österreichischen Ost- und Südosteuropa-Instituts, number 11.) Vienna: Hermann Böhlaus. 1983. Pp. 301. DM 74.

This book deals with Joseph II's struggle with the Hungarian local governments over the matter of military recruitment in Hungary, chiefly at the time of the Turkish War of 1788–89. In a larger sense it attempts to provide a picture of the emperor's overall relations with this, his most obstreperous possession. Horst Haselsteiner provides the first really coherent account, in a Western language, of how Joseph's administrative reforms, particularly that of 1785, worked in his Hungarian dominions: for the most part they worked very well, until his serious mistakes, military as well as political, during the war opened the door to various dissident elements in the country.

The principal idea behind the reform of 1785 was to consolidate the numerous provincial administrative districts into ten large ones. These were presided over by men Joseph trusted, drawn mostly from the ranks of the so-called *bene possessionati*, nobles not quite as rich as the two hundred or so magnates but sufficiently influential to dominate the local assemblies of the various districts over which they presided. The system initially worked. In the first general levy, in 1787, two-thirds of the demanded recruits were under arms by the end of the year and practically all by the start of the campaigning season the next spring. Although a somewhat more rapid recruitment process might have enabled the Austrians to attack the Ottomans in conjunction with the Russian campaign in the late autumn of 1787, the point is moot because, for a variety of good reasons, Joseph had at that point not yet decided to enter the war.

In Hungary there was a good deal of grumbling about the unconstitutionality of Joseph's procedures, his refusal to summon the *Landtag*, and his arbitrary decision to employ the *insurrectio* (the Magyar feudal levy) in a campaign ouside of Hungary's territorial limits. But none of this had a

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seriously threatening character until it became abundantly evident that the campaign was being badly botched and that Joseph himself was mortally ill.

Haselsteiner argues that, when genuine opposition did arise, it was generally stronger in those regions with a relatively large number of nobles—in some cases, over 7 percent—in the population, in the areas where the percentage of Protestants was highest, and in the northeast, where the Ottoman threat was presumably not as clearly perceived. Although these are all commonsense conclusions, it is no doubt useful to have them demonstrated on the basis of solid archival research in Vienna and Budapest. Somewhat less convincing are Haselsteiner's attempts to demonstrate a link between Protestant ideology—grounded in Calvinistic autochtonous sentiments—Freemasonry, and oppositional elements in Hungary. Here he follows the printed literature and fails to advance the argument very far.

In general, this is a sound piece of work. What is particularly welcome is the methodical thoroughness with which quantitative data—touching, no matter how remotely, on the principal arguments—are supplied. This may strike some readers as useless pedantry, but for the nonspecialist in Hungarian affairs it is extremely useful to have this information readily at hand.

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JOHN F. N. BRADLEY. *Czech Nationalism in the Nineteenth Century*. (East European Monographs, number 157.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. xii, 153. $20.00.

This short book, based on the revision of a doctoral thesis prepared in the late 1950s, has the praiseworthy purpose of studying "the development of Czech nationalism between 1880 and 1914 and in particular the part played by T. G. Masaryk" (p. ix). Alas, it does not do justice to this important theme, least of all to Masaryk's role. Masaryk, who is dealt with in a final chapter of three pages and in other scattered references, is depicted as "concentrating on cultural affairs" (p. 97). John F. N. Bradley ignores Masaryk's extraordinary influence on pre-1914 Czech politics as a historian, professor, journalist, and lecturer, as a ceaseless critic of Austrian policies and Czech political tendencies, and as a member of the Austrian parliament for a number of years.

The all-too-cursory treatment of Czech nationalism is often inaccurate. The analysis of "integral nationalism" of the 1880s, for instance, makes almost no reference to the crucial issues of "states' right," or autonomy and federalism, and fails to distinguish the substantial differences in tactics between Young and Old Czechs. After the former had "achieved power in 1890," they were "busy hatching coup d'états in Vienna" (p. 46) and adopted "the technique of mass rioting" (p. 33). But nothing is said of their "positive" policy of participation in the imperial parliament and administration.

A chapter on "social radicalism" gives the impression that the Social Democrats and the "nationalists" (not otherwise identified) were deeply involved in "public rioting" for universal suffrage. Bradley includes no discussion of the serious efforts that were made by various individuals and groups—Tomáš Masaryk, Josef Kaizl, Karel Kramář, the "progressives" of various tendencies, the National Socialists (who are scarcely mentioned), and the Social Democrats—to grapple with the "social question" or to reconcile nationalism and socialism. A chapter on Catholicism deals with Hussitism and the Cyrillo-Methodian movement, as well as with Masaryk's anticlericalism, but does not discuss the Czech Catholic political parties.

The author's treatment of the "external politics" of Czech nationalism includes the extraordinary statement that all Czech politicians, except Masaryk, "believed in some sort of Pan-Slavism." This ignores the critical attitudes toward Russia and pan-Slavism by many Czechs other than Masaryk, the fundamental distinctions between Austro-Slavism, Russophilism, and pan-Slavism, and the differing and changing positions taken by Czech politicians toward Austrian foreign policy, Russia, and the Balkan Slavs.

Bradley's documentation is inadequate and often confusing. Books published from the 1960s—such as those by Bruce Garver on the Young Czechs, Paul Vyšný on neo-Slavism, and Otto Urban on Czech society and politics from 1848 to 1918—are not cited, so one assumes there was no systematic updating of research since the writing of the original thesis. Archival sources are used, but often indiscriminately and with the original bias retained (for example, references by police authorities to "rioting"). Bradley includes quite useless listings of the topics dealt with in specific archives. Footnotes often fail in their purposes; Bradley cites, in bunches, many sources, some of which have no apparent relevance to the subject, and often neglects to give the exact reference for a citation. The publishers must share responsibility with the author for such editorial lapses, as well as, of course, for deciding to publish a book of such poor quality.

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CONDOLEEZZA RICE. *The Soviet Union and the Czechoslovak Army, 1948–1983: Uncertain Allegiance.* Princeton: Princeton University Press. 1984. Pp. xiv, 303. $37.50.

To write a scholarly study on the relationship of the Soviet Union and the Czechoslovak army without access to relevant Czechoslovak and Soviet documents is difficult. Therefore, much of this book by Condoleezza Rice is based on secondary works. His thesis is that the Soviets directly influence military elites in the satellite countries, in addition to the Soviet Communist party interacting with the domestic parties. Rice selects Czechoslovakia as a case study and attempts to show the role of the military as instrument of both national defense and the Soviet-controlled military alliance.

Rice's selection of sources raises questions, since he frequently does not sift facts from propaganda and valid information from disinformation or misinformation. He passes judgments and expresses opinions without adequate knowledge of facts. It does not add to his credibility when he uses a source written by Josef Hodic; Rice fails to notice that this "former military scientist" (p. 99) was a communist agent who returned to Czechoslovakia several years ago. Rice based his discussion of the "Šejna affair" (pp. 111, 116, 144) largely on communist propaganda sources and did not consult writings and statements by former General Jan Šejna who had access to Warsaw Pact documents and is the highest military officer from the Soviet bloc to defect to the West since World War II.

Rice's generalizations reflect his lack of knowledge about history and the nationality problem in Czechoslovakia. For example, in 1955 Czechoslovakia was not yet "the Czechoslovak Socialist Republic" (pp. 83, 84). In May 1938 Ludvík Svoboda was serving in the Czech army, not organizing a Czech military unit in Poland. In the fall of 1939 he was captured by the Soviet invading forces in eastern Poland; he did not "[escape] to the USSR" (p. 43). Rice's discussion of the "Czechoslovak Legion" that was "born during the chaotic period preceding the fall of the Russian empire" (pp. 44–46) is ridiculous. (It was "born" on September 28, 1914.) He is clearly ignorant of the history of the military unit as well as of the geography of the area on which it fought.

Rice claims that "Czechoslovaks are supposedly passive and consider resistance to invading forces unnecessary and dangerous, preferring instead political solution" (p. 4). First, there are Czechs and Slovaks but no Czechoslovaks. Second, history shows that Czechs resisted the invading Prussians in 1866, that during World War I more than one million Czechs and Slovaks fought in the Austro-Hungarian armies, and that several tens of thousands volunteered for service in the Czechoslovak armies in Russia, France, and Italy. In 1919 Czechs and Slovaks fought the invading armies of Béla Kun in Slovakia. In 1939 and 1948, "the Czechoslovak president, Edvard Beneš, ordered his troops to the barracks," writes Rice. "[Alexander] Dubček and Svoboda were, then just following precedent. Czechoslovak passivity meant that the decision of 1968 was preordained" (pp. 4–6). Nothing, indeed, is preordained in history. Moreover, Beneš in 1939 was no longer president but was teaching at the University of Chicago.

In comparing Poland in 1981 and Czechoslovakia in 1968, Rice does not mention the obvious: whereas Soviet troops have been garrisoned in Poland since the end of World War II and, therefore, an invasion of Poland was unnecessary, the main objective of the 1968 invasion of Czechoslovakia was to force Dubček's regime to accept the stationing of Soviet troops in the country.

The writing abounds with meaningless phrases, such as is its "last word": "Thirty-five years after its creation, the Czechoslovak People's Army stands suspended between the Czechoslovak nation and the socialist world order" (p. 245).

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THOMAS M. BARKER. *The Slovene Minority of Carinthia.* Assisted by ANDREAS MORITSCH. (East European Monographs, number 169.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. 415. $35.00.

Close to seven decades after the dissolution of Austria-Hungary, deep wounds still remain on the living flesh of numerically small Central European nationalities, whose political and cultural unity was disrupted in the wake of the First World War. The Slovenes, one of the Slavic nationalities that lived compactly in the Habsburg monarchy before 1918, were separated by the new frontiers between the emerging successor states of Yugoslavia, Austria, Italy, and Hungary. The bulk of the Slovenes found themselves in Yugoslavia, where they developed their national identity and political influence as one of the constituent nationalities of this multinational South Slavic state. Although the northwestern frontier of Yugoslavia underwent major changes after the Second World War, the border with Austria was not affected, making the situation of some fifty thousand Slovenes in Carinthia (the homeland of most of Austria's Slovenes) in effect permanent. The current policy of Yugoslavia, which reflects the stand of the Slovene leadership in Ljubljana, is to develop relations with neutral Austria in such a way as to deemphasize the importance of the frontier.

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Thomas M. Barker's admirable book elaborates all the obstacles in the way of normalization, the Slovene position in Carinthia being historically a case of centuries-long retreat in the face of Austro-German expansion and national assimiliationism.

The product of several decades of intense research on the history of Carinthia and Carinthian Slovenes, this excellent book expands Barker's earlier work, *The Slovenes of Carinthia: A National Minority Problem* (1960), but not in any mechanical way. He generously credits the assistance of a number of scholars from Yugoslavia, Austria, and the United States, most notably, Andreas Moritsch, whom Barker acknowledges as his collaborator. The value of the book is enhanced by a series of excellent maps by the scholarly team of Vladimir Klemenčič and Matjaž Klemenčič. Barker traces the history of Carinthia from the early medieval tribal duchy to its present status, giving considerable space to a number of tangled historiographical problems, notably, on the nature of Carinthian autonomy. He is extremely effective in several genres: always in total control of his material, he elegantly juggles diplomatic and political history, historical demography, and the analysis of contemporary politics. His book will be a valuable addition to the collections of Central European and South Slavic specialists, students of minority affairs, and political analysts.

The core of Barker's book deals with the attempts of Carinthian Slovenes to survive as a national group in the hostile environment of post-1918 Austria. Even if one sets apart the genocidal policies of the (Austrian) Nazis after the Anschluss as exceptional, the fact remains that under the best of circumstances—notably, after 1945—the Slovenes continue to be exposed to calculated assimilatory pressures, either by way of outright Germanization or under the guise of ersatz "Windisch" status that would sever their links with the Slovene matrix in Yugoslavia. Ljubljana's longstanding practice of ideological polarization among the Slovene minorities in Austria and Italy has worsened matters. Recently Ljubljana has struck a more conciliatory note that would affirm the unity of the Slovenes on a nonideological and trans-Yugoslav basis. This and the recently expressed self-critical statements by the former Austrian Chancellor Bruno Kreisky on the faults of his own policies toward the Austrian Slovenes are two important steps that might assure that Carinthia will not enter the twenty-first century without a people that settled its basin well over fourteen centuries ago.

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DAVID CONSTANTINE. *Early Greek Travellers and the Hellenic Ideal.* New York: Cambridge University Press. 1984. Pp. xii, 241. $49.50.

This excellent book has two themes. The first is to describe the most important and interesting travelers in Greece in the seventeenth and eighteenth centuries; the second is to examine the development of Western Hellenism—the "idea of Greece"—during the same period. David Constantine begins with Jacob Spon's and George Wheler's travels in the 1670s, proceeds to those of Edmund Chishull and J. P. de Tournefort, and devotes a whole chapter to Robert Wood. He then moves from the travelers to those who thought or dreamed about Greece from afar: Johann Joachim Winckelmann is the centerpiece, preceded by half a chapter given to C. G. Heyne and followed by another half-chapter on Winckelmann's disciple J. H. von Riedesel, the only German of any consequence actually to visit Greece during the period under discussion. The remaining chapters examine P. A. Guys, the effect of the insurrection of 1770 on European attitudes, and Richard Chandler's expedition to Ionia.

As Constantine observes at the start, the travelers in Greece were mostly French or British, but the scholars and poets whom he studies for the expression of the Hellenic ideal were German. One might expect the book to break awkwardly into two, but the author relates his themes to each other with much dexterity. We watch Heyne in Göttingen reading Wood, Guys in Hellas rhapsodizing in the manner of Winckelmann. Constantine has not only an eye for telling detail but also a nice sense of period. For example, he sees Chishull as having some characteristics that look back to early seventeenth-century travelers like George Sandys, others that look forward to the nascent romanticism examined in the later chapters. Yet Constantine manages to place Chishull just where he belongs, at the point where the seventeenth century gives way to the eighteenth. Ending with Chandler may seem an arbitrary choice, but Constantine makes a plausible case for its appropriateness: Chandler was one of the last of the amateurs (in the best sense of that word) and one of the first of the professionals.

A charm of this book is the combination of an elegant, often informal style with an infectious enthusiasm for the men discussed. Few of Constantine's subjects were men of profoundly original mind, but he claims to see in almost all of them an individuality of response that he complains of not finding in their nineteenth-century successors. What he likes in them is the merit of his own book: enthusiasm and enjoyment (though he surely lets Winckelmann get away with too much on this account). It is gratifying to find Wood accorded his due and to find his place in the history of classical scholarship so well understood. Nonetheless (for no academic review is complete without some carping), he is vague about Heyne and does not understand

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his significance. But this is an admirable book—lucid, shrewd, and a pleasure to read.

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R. E. F. SMITH and DAVID CHRISTIAN. *Bread and Salt: A Social and Economic History of Food and Drink in Russia.* New York: Cambridge University Press. 1984. Pp. xvi, 391. $59.50.

This unusual book fills a void in the field of Russian history that really needed filling. In nine well-researched and for the most part very readable chapters spanning a period from the sixteenth through the nineteenth century, R. E. F. Smith and David Christian cover such diverse topics as changes in the diets of the Russian population; the influence of foreign food, drink, and utensils; the impact of government fiscal and regulatory policy on the production, distribution, and consumption of alcoholic beverages; and patterns of feast (all too rare) and famine (all too frequent, even on the eve of the twentieth century). Much of this discussion is nicely differentiated by region, by social class, and, of course, by historical period and emphasizes continuity at least as much as change. There are many excellent, easily digestible statistical tables, some wonderful illustrations from woodcuts, paintings, and photographs, and numerous well-chosen, often highly entertaining quotations from original sources, including accounts of foreign travelers.

Despite the subtitle, this book is not a social history in the sense suggested on the dust jacket. By and large, the authors do not approach eating and drinking "as social activities." We rarely observe Russian peasants—let alone workers, merchants, or nobles—actually engaged in mealtime activity or leisure-time drinking, nor are eating and drinking sufficiently explored in connection with other social and cultural activities, although some interesting comments are scattered throughout the book on the relations between food, drink, and religious celebration. Rather, this book should be seen as a cross between an *Annales*-type study of broad, long-term trends in the material culture of Russia in the areas of food and drink and a more traditional history of the relevant government policies. Viewed in this light, it is an attractive and original book.

Although this is the combined effort of authors who declare themselves "jointly responsible for the book as a whole" (p. 2), chapters 1 through 6 were written by Smith and chapters 7 through 9 (mainly on the nineteenth century) by Christian. On balance, the latter chapters are more successful. They are better organized and more polished, following a clearer line of exposition than the earlier sections, which are somewhat patchy and not as well integrated thematically. Nevertheless, the book is satisfying as a whole. Both as a pioneering introduction to an important subject and as a very useful reference work—its thorough index contains items as diverse as "Bears, tamed" (not a food) and "Pretzels" (a food)—it belongs on the shelves of all students of Russian history. Although certain scholars may complain that inadequate space is devoted to their favorite foods—the coverage of borscht, for example, is very thin—the authors, who modestly describe their book as an "eclectic survey of some aspects of food and drink in Russia," richly deserve our gratitude for this very valuable (and very expensive) study.

REGINALD E. ZELNIK
University of California, Berkeley

A. I. KOPANEV. *Krest'iane russkogo Severa v XVII v.* [The Peasants of the Russian North in the Seventeenth Century]. Leningrad: Nauka. 1984. Pp. 242. 3 r. 20 k.

This volume continues A. I. Kopanev's earlier work, *The Peasantry of North Russia in the Sixteenth Century* (1978), into the seventeenth century and is similar in general approach and the nature of the materials examined. Considerable attention is given to the forms of and developments in peasant landholding, to the hamlet and some interesting material on land allocations, and to peasant occupations. Social differentiation—the development of both rich peasants with considerable holdings and peasants who became sharecroppers or laborers—is regarded as evidence of the emergence in this region of protocapitalism. Finally, examination of one *volost'* shows both how the central state offices administered the region at no cost to the center and how this imposed a burden on the locality.

The seventeenth century in Russia started with a disastrous famine lasting three years and with invasions by Poles and, to a smaller extent, Swedes. Effectively it was not until the 1620s, after the election of Michael Romanov to the throne, that reconstruction could be undertaken. The disasters of the early seventeenth century had their impact on the North, which was then of great importance, partly because it included Russia's only seaboard. Peasant mobility and migration increased, and the emergent state associated with the enserfment of the peasants (established by the Code of 1649) grew in authority. But the taxpaying peasants in the area remained free.

Kopanev adduces much material showing that, in reality, peasants continued to sell, bequeath, mortgage, and donate "their" land despite the law. They

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continued actively to defend these established customs against the erratic processes of the tsarist state and against encroachments by local landowners, mainly monasteries. This peasantry, free in fact, was active in developing farming and related gathering activities in the harsh climate and physical conditions of the area. Although the salt trade, an essential adjunct of fishing, declined in the seventeenth century both as a peasant occupation and in quantities produced, it remained an important source of demand for labor, mainly for the provision of fuel and for transport. Sea-fishing became important. Such activities not merely supplemented farming but also provided business opportunities for some, created a labor market for many others, and encouraged the local development of many crafts and trades in the villages. In this sense they contributed to "elements of capitalism," though within the framework of a serf-based society.

The book provides much useful and interesting detailed material, some from archival sources. It enhances our knowledge of the primary producers in this area of seventeenth-century Russia and touches on the vexed question of an alternative development to that of the serf-based economy and society. At times the Western reader might wish for more tabulation to make comparisons easier and to disclose gaps. Maps would also be an advantage. But, of course, comparable Western studies would be much more costly.

R. E. F. SMITH
University of Birmingham

EDWARD C. THADEN. *Russia's Western Borderlands, 1710–1870.* Assisted by MARIANNA FORSTER THADEN. Princeton: Princeton University Press. 1984. Pp. xi, 278.

The Russian empire in the eighteenth and nineteenth centuries is traditionally characterized as a highly centralized autocratic state, but that must be strongly qualified with respect to the peripheral regions inhabited by peoples of different language, religion, and customs. The particular merit of Edward C. Thaden's book is to call our attention to the diversity of the empire and to the inconsistent policies pursued by the central government in the borderlands.

Thaden deals only with what were called the Western provinces—the territories of the former Grand Duchy of Lithuania, the three Baltic provinces, Congress Poland, and the Grand Duchy of Finland. He reviews the relationship between each province and St. Petersburg in three periods: the eighteenth century; the years 1796–1830, when the imperial government pursued policies of local administrative autonomy; and, finally, the years 1830–70, when policy alternated between centralization and decentralization. The fourth period (1870–1917), which Thaden dealt with in previous works, was characterized by more or less systematic "Russification."

Thaden's comprehensive study of the ambivalent relationship between the central authorities and local government is based on an impressive array of published primary sources and monographs in practically all the relevant languages. His reasonable conclusion is that the imperial administration preferred to deal with local elites and constituted bodies wherever these were clearly in control and acquiescent to the interests of the Russian empire. Thus, the Baltic nobilities, the Swedish-Finnish governing strata, and even the Polish *szlachta* were allowed to retain not only their privileged socioeconomic position but also their traditional role in local administrative institutions. But, as these autonomies began to hinder St. Petersburg's goals, they were increasingly restricted or eliminated altogether. This occurred first, and with particular force, in the former Lithuanian and Polish territories. The Baltic provinces and Finland kept their special arrangements longer since their elites not only remained loyal to the empire but also contributed directly to its political and cultural progress. Of course, one of the main reasons for this relatively cautious policy was St. Petersburg's perennial lack of qualified personnel to take over all aspects of local administration.

This resulted in a paradoxical situation: as the locally constituted elites' resistance to administrative unification grew, the imperial government resorted to encouraging the educational and socioeconomic progress of the lower classes, and this promoted burgeoning nationalism. This sparked the renaissance of a literary culture among the Baltic and Ukrainian peoples. As a consequence, in the last quarter of the ninteenth century the imperial regime lost the loyalty and support of the traditional ruling elites through its policy of administrative uniformity, while promoting among the lower classes the very forces of nationalism that eventually helped destroy the integrity of the empire.

Thaden's account is full, clear, and balanced, although the story of the formerly Lithuanian lands has often been told in greater detail from a Polish or Ukrainian perspective. Although he recognizes the crucial importance of educational and cultural developments for a comprehensive explanation of imperial policy, his treatment is too sketchy, except of Finland, whose history and sources he knows particularly well. What I miss most in this otherwise useful book is an explanatory analysis of imperial administrative policy and reform endeavors within the broader framework of Russian imperial concerns and political practice. Other readers may wish for a fuller and more specific treatment of the social

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and economic context within which the political and administrative story unfolded. The book could have benefited from another proofreading and check of the bibliography, which unaccountably omits titles cited in the footnotes.

MARC RAEFF
Columbia University

HANS DIETER VON ENGELHARDT and HUBERTUS NEUSCHÄFFER. *Die livländische gemeinnützige und ökonomische Sozietät 1792–1939: Ein Beitrag zur Agrargeschichte des Ostseeraums*. (Quellen und Studien zur Baltischen Geschichte, number 5.) Cologne: Böhlau. 1983. Pp. ix, 215.

This book, which is the fifth volume in the series of sources and studies of Baltic history produced by the German Commission on Baltic Studies, deals with the history of the Public Utilities and Economic Society of Livonia as a contribution to the studies of agrarian history of the region around the Baltic Sea. The rather thin book consists of two uneven parts, the first covering the years 1792–1918 in 144 pages and the second covering the years 1918–39 in only 20 pages. The book also contains a rather good bibliography, an index of persons mentioned, the verbatim texts of some important documents, and a few rare photographs. The bulk of the research has been done by a young German historian, Hubertus Neuschäffer. The groundwork was prepared, however, by Hans Dieter von Engelhardt, the last secretary of the Public Utilities and Economic Society of Livonia; he also produced the thin second part of the book as his own report.

Ancient Livonia covered northern Latvia and southern Estonia. The organization followed the example of the Society of Improvers in the Knowledge of Agriculture in Scotland, founded in 1723; the Royal Society of Agriculture in London, founded in 1753; the Swiss agrarian cantonal society in Bern, founded in 1759; and various similar German, Russian, and Scandinavian societies that appeared from 1763 to 1792. The society was born in Riga, but its seat was transferred to the Estonian university town of Tartu (Dorpat) in 1813 in the aftermath of the Napoleonic campaigns in the Baltic region during the previous year. The original members of the society were well-meaning Baltic German nobles who had been influenced by the Age of Enlightenment and the scientific agrarian discoveries in Western Europe. They were interested not only in advancing knowledge regarding improvements in agriculture but also in fostering the general well-being of the population in their own provinces.

Their activities are discussed briefly in rather dry prose, and such important phenomena as serfdom and its abolition, the great revolution of 1905, the proclamation of Estonian and Latvian independence, and the shattering agrarian reformation in these countries in 1920 are covered in only a few sentences. The membership of the society, however, included some of the best minds found in the former Livonian upper class of Baltic German nobles and literati, whose well-meaning activities deserve a place in Baltic cultural and economic history. Nevertheless, their accomplishments are described too sparingly in this book, which concentrates on the general activities and directions of the society.

EDGAR ANDERSON
San Jose State University

CYNTHIA H. WHITTAKER. *The Origins of Modern Russian Education: An Intellectual Biography of Count Sergei Uvarov, 1786–1855*. Dekalb: Northern Illinois University Press. 1984. Pp. x, 348. $29.00.

Since the publication of Patrick L. Alston's pioneering work, *Education and the State in Tsarist Russia* (1969), Western historians have begun to penetrate beneath the passionate but too-often sweeping condemnations of prerevolutionary Russian educational policy to discover a considerable commitment to education on the part of frequently maligned ministers. Cynthia H. Whittaker's book clearly belongs in this tradition. Based on extensive research in the printed sources and in Soviet archives, her carefully argued and thoroughly documented study chronicles Sergei Uvarov's lifelong struggle both as St. Petersburg district superintendent (1810–21) and as education minister (1833–49) to further the cause of learning in Russia, a struggle that, she claims, in the end produced "an educated public" and the "trained personnel" necessary to implement the Great Reforms of the 1860s.

The task of spreading education in Russia was immense. Backward, lacking in material and human resources, and with more than half its population in bondage, Russia had not even received a full educational system until 1803–04. Moreover, many powerful figures, including Emperor Nicholas I himself, considered learning a mixed blessing at best and singled out the schools as one of the major causes of unrest in the West. Out of these conditions arose the question that bedeviled all conscientious tsarist education ministers: how to employ education for Russia's much-needed development without undermining imperial absolutism or, in Whittaker's words, how to achieve "an accommodation of autocratic politics with learning" (p. 230).

Uvarov was the first education minister fully to recognize this dilemma. On the one hand, he openly affirmed the power of ideas, culture, and education to transform people and hence assist Russia's tran-

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sition to a freer society. Indeed, Uvarov's accomplishments were considerable, especially in the establishment of the classical gymnasium, the rapid improvement of the universities, and the reinvigoration of the Academy of Sciences. On the other hand, to counter education's "negative" consequences, Uvarov relied on a policy of ministerial centralization, of increased control over students and curricula, and of attracting the elite classes to secondary and higher education. Yet, despite his successes and close ties with the court, Uvarov eventually fell victim to the reactionary backlash in Russia after 1848, his vigorous defense of the universities only isolating him further from the tsar. All his successors were to face the same dilemma, which became compounded by the need to assimilate the emancipated serfs, to supply the manpower demanded by an industrializing society, and to quiet an ever more restless student body.

Although Whittaker's arguments may sound rather familiar to experts on Russian education, her judicious discussion of Uvarov's policies, which relates them to his own intellectual development and to educational practices in Europe, enables the reader to appreciate Uvarov's achievement. Still, this reviewer would have preferred her to venture off the beaten track to explore newer, more theoretical problems in Russian educational history such as the role of a classical education in the acculturation process, the precise relationship of education to modernization, and, indeed, the meaning of the term "education" in pre-emancipation Russia. The author is certainly aware of these questions; one hopes her future studies will address some of them.

ALLEN SINEL
University of British Columbia

V. A. NARDOVA. *Gorodskoe samoupravlenie v Rossii v 60-kh–nachale 90-kh godov XIX v.: Pravitel'stvennaia politika* [Municipal Self-government in Russia from the 1860s to the Beginning of the 1890s: Governmental Policies]. Leningrad: Nauka. 1984. Pp. 357. 3 r.

This sober, scholarly book offers a wealth of information on the evolution and operation of city government in nineteenth-century Russia. Refreshingly, only a few citations from Lenin are sprinkled at the beginning and conclusion; socioeconomic jargon and references to Marxist stages of history are absent. Maintaining that the tsarist government found city government inadequate, mainly because nobles were underrepresented, V. A. Nardova surveys experiments in St. Petersburg, Moscow, and Odessa from the 1840s to 1860s. She thoroughly reviews debates within grass-roots and governmental commissions that formulated the general reform of city government in 1870, implemented nationwide over a fifteen-year period.

The core of the book is fascinating. To illustrate the complexity of electing the duma, *uprava* (executive committee), and mayor with the new three-curia system, Nardova focuses—perhaps unfairly—on the 1873 St. Petersburg election. Every stage was fraught with difficulty: registering voters, the number of candidates (1,555), the dearth of ballot boxes, long voting days, a nine-day election period, falsification of ballots. That women could vote by proxy is interesting.

Equally absorbing is her analysis of the first five elections in forty-six cities. Records in the Ministry of Internal Affairs, reproduced in numerous tables, contain no information on landholdings of voters but only on whether they paid taxes on immovable property or from trade and industrial activity. Nardova tries to correct this with data on the landholdings of duma representatives and mayors. Still, she persists in calling those who paid taxes on immovable property nobility even though the group included *raznochintsy* and clergy. Also, although suffrage requirements varied from city to city, she never defines the "*meshchanstvo*, artisans, and peasants" who constituted the third curia. Nevertheless, her conclusions are substantial. Despite the government's ostensible desire to strengthen the nobility and despite the predominance of owners of immovable property among voters, those in the "trading-industrial" category were the most active voters, and merchants received the highest percentage of representation in city dumas. In general, about equal numbers of merchants and landowners were elected mayors, although merchants predominated in non–Black Earth regions and Siberia. Surprisingly, in *uezd* cities some mayors were *meshchane* and "lower classes" received representation.

This book is as much about the central government as about cities. According to her revisionist account, if Russian cities never really attained self-government, the central government at least nurtured fledgling city government in the 1870s and early 1880s, when conflicts occurred between cities and local administration. Ministers Durnovo and Tolstoi rebuked governors for violating city self-government. The State Council basically supported self-government, and the Senate consistently ruled in the cities' favor when they clashed with ministers in the 1880s. Nardova concludes that the Kakhanov Commission of the 1880s aimed to make local self-government more rational rather than independent.

In an intriguing finale Nardova portrays the 1892 law, which limited urban suffrage, as a government response to city complaints about the requirement that duma candidates receive a majority; often this

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necessitated second elections and left duma seats unfilled. In sum, the endless debates, commissions, and adjudication reveal an effective, pluralistic central government.

There are minor deficiencies in this book. In addition to Nardova's lack of preciseness in labeling urban voters, she consistently refers to lodgers—for whom repeated attempts were made to get the vote, on payment of a special apartment tax—as educated professionals. She only sketchily discusses city budgets although obligatory, peripheral expenditures were a source of great friction between city and central government. There may be more information on provincial and *uezd* city governments outside the capitals and major cities, on duma debates, and on relations between *uprava* and duma. City government in tsarist Russia is, however, a vast subject; future local studies will produce a detailed mosaic. Nardova's work is solid and provocative, reflecting monumental research and casting light into little explored corners of the tsarist government.

MARY SCHAEFFER CONROY
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A. J. POLAN. *Lenin and the End of Politics*. Berkeley and Los Angeles: University of California Press. 1984. Pp. viii, 229. Cloth $22.50, paper $9.95.

"Few people will read this book," A. J. Polan writes at the conclusion of his study, "because, apart from whatever inherent defects it may possess, few people in the world we live in are yet much interested in such matters" (p. 219). Polan's pessimism may be justified, but he himself has not helped his cause. He advances an original and provocative thesis about Lenin's political thought, but his dazzling display of erudition, which occupies an inordinate amount of space in this slim volume, is likely to baffle even the most learned specialists on Russian radicalism.

Polan mentions or discusses, among other things, the views of Alexis de Toqueville, Max Weber, Wilhelm Dilthey, Thomas Jefferson, Theodor Adorno, Louis Althusser, Jürgen Habermas, Herbert Marcuse, and Jean-Paul Sartre without always making clear the relevance of these excursions into intellectual history to Lenin's doctrines. Moreover, scholars interested in Leninism are unlikely to find Polan's convoluted and misty prose appealing. Two examples will suffice. After mentioning the "Corrigan-Ramsay-Sayer thesis" on the "collapse of politics into administration," he writes, "In the light of the terrorism of this concept, it cannot be repeated too often that without a genuine process of discursive will-formation, there is no politics that merits the name, and there is no democracy that is not a travesty of the meaning the concept holds" (p. 97). He explains the seductiveness of Lenin's *The State and Revolution* with the following words: "It speaks to the consciousness of lack, and translates it into a consciousness of *loss*: that is, it promises an end to the fundamental anguish of being, that of being in time" (p. 206).

Actually, Polan's central theme is quite straightforward. He contends that scholars have been mistaken in dismissing Lenin's *The State and Revolution*, his most utopian work, as merely a tactical tract written in 1917 to further the cause of bolshevism during the revolution. Polan believes that it was by far Lenin's most important publication because it accurately depicted the kind of society he favored and that he and his successors established. In rejecting the distinction between the executive, legislative, and judicial arms of the state, in advocating a polity in which all political functions were to be centered in one institution (the soviet), and in failing to provide for the rule of law, Lenin repudiated democratic politics and laid the basis for a tyrannical system of government. Although he claimed to favor the liberation of the masses, he "bears . . . the responsibility for the Gulag" (p. 130).

Most scholars would probably disagree that one of Lenin's works can be burdened with so heavy a responsibility for the political evolution of the Soviet Union in the 1930s, 1940s, and beyond. They might even question whether Lenin's entire corpus can be assigned so critical a role. Had Polan offered a more systematic and lucid exposition of his thesis, he might have made an interesting contribution to the ongoing debate about the origins of the Soviet system of government. As it stands, the book will probably be given short shrift.

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City University of New York

JOHN D. BASIL. *The Mensheviks in the Revolution of 1917*. Columbus, Ohio: Slavica. 1983. Pp. 220. $18.95.

Of all the political groups that contended for influence over the course of the Russian Revolution, the Menshevik Social Democrats remain the least studied. Bolshevik victory not only doomed the Mensheviks to be viewed as weak and inconsequential but also deprived Western historians of access to the documents—party archives, conference minutes, and newspapers published outside of Moscow and Petrograd—that might tell the story of the Mensheviks' role in the revolution. At best, historians can rely on only a few newspapers and a handful of memoirs. In these circumstances, John D. Basil deserves some credit for reconstructing the general

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course of Menshevik policies in 1917 and for making the point that Menshevik doctrines combined the West European ideology of Marxism with an original analysis of Russian society and institutions. Beyond this, however, he fails to explain satisfactorily the early success and subsequent defeat of the Mensheviks in 1917, and he also makes several serious blunders.

While arguing for the role of ideology in determining Menshevik political positions, Basil neglects the ideological evolution that occurred after 1905. He does not use the important publications of those years (*Golos sotsial-demokrata*, *Nasha zaria*) or the private correspondence between leading Menshevik figures, where the lessons of the first revolution were analyzed and a distinct Menshevik revolutionary strategy formulated. Similarly, he does not study the wartime writings of I. G. Tsereteli, whom he correctly identifies as the principal leader of menshevism in 1917. Instead, Basil assumes that Menshevik strategies in 1917 grew directly from concepts developed by G. V. Plekhanov (in the 1880s) and P. B. Axelrod (on the eve of 1905). Of these concepts, he relies on Plekhanov's view of Russia as "Asiatic"—a concept no Menshevik seriously entertained after 1905 and only A. N. Potresov used in 1917. He also ignores Tsereteli's guiding strategy of uniting "the vital forces of the country." In fact, the differences among the factions and subgroups of menshevism, as well as those views that held them together, are not at all clear, and even the important role of the "practicals" is not mentioned. Menshevik figures, too, are improperly or wrongly characterized: why was M. I. Skobelev, the minister of labor, "narcissistic," and how could F. I. Dan, the best organizer and most formidable politician among the original Mensheviks, be described as "a man capable of neither creative and clear thinking nor decisive leadership" (p. 165)?

There are further deficiencies in Basil's treatment of the social and political context in which the Mensheviks worked in 1917. Workers, peasants, and soldiers remain largely invisible, yet Basil makes certain assumptions about their sentiments and behavior, and these, in turn, determine his judgment of the Mensheviks. The "masses," he believes, would have gladly accepted a policy of full cooperation between socialists and "liberals" if only the Mensheviks had not allowed their own doctrinaire hatred of the bourgeoisie to overcome "practical politics" (pp. 71–72). "The confusion of July" is Basil's description of the demonstrations on July 3–4 during which workers and soldiers expressed their distaste for coalition (pp. 103–04). Even the fatal economic crisis of the summer and fall is judged to have been merely a "technical problem," for which a solution could easily have been found had the Mensheviks only agreed to collaborate with the liberals in good faith (p. 161).

In sum, Basil's lack of appreciation for the complexities of the revolution and his haphazard treatment of ideology leave the political strategies over which the Mensheviks agonized unexplained. How did Mensheviks who had supported dual power in March 1917 come to endorse coalition in May? (Contrary to Basil's assertion there were many Internationalists among them.) Why would a champion of collaboration with the "liberal bourgeoisie" (B. O. Bogdanov) turn against this policy in September (pp. 151–52)? What alternatives did the Mensheviks consider once coalition became impossible? (Dan's "homogeneous democratic government" and Iu. O. Martov's "homogeneous socialist government" are not mentioned.) Both the revolution and the Mensheviks are worthy of a more informed consideration, one that would convey the magnitude of the problems created by the revolution while also appraising the ideological and personal factors that limited the Mensheviks' capacity to confront these problems.

ZIVA GALILI Y GARCIA
Rutgers University

THOMAS F. REMINGTON. *Building Socialism in Bolshevik Russia: Ideology and Industrial Organization, 1917–1921*. (Series in Russian and East European Studies, number 6.) Pittsburgh: University of Pittsburgh Press. 1984. Pp. x, 222. $19.95.

Scholars continue to debate the nature of the early Soviet political system. Was it the conscious result of planned policy or the unintended product of large-scale experimentation? Was War Communism a fully articulated ideological system or a collection of improvised actions? Thomas F. Remington seeks to bypass the design-versus-accident debate by examining the evolution of Soviet economic organization from the theoretical perspective of state-society relations. His book uses the history of Soviet industrial organization during the Civil War in order to examine the proposition that the state constructed socialism by mobilizing previously autonomous elements of society into the state and that, in so doing, the state destroyed its social base, thereby destroying its ability to function. The key assumption here is that "it was the attempt to build the new state by means of mobilizing society that prevented the regime from establishing the self-sustaining social institutions on which a socialist regime depends" (p. viii). Unfortunately, little discussion follows about what independent social institutions a socialist regime ought to have, and this assumption thus fails to act effectively as an organizing principle of the subse-

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quent examination of policy and practice in the area of industrial organization.

This theme of state-society relations runs well beneath the surface in the substantive chapters of the book, which ably describe factory committees and workers' control, the origins of the institutions erected to administer socialist industrial production, and the subsequent debates and policies concerning the mobilization of labor (presumably the key element in the state's devouring of civil society). Remington's sources include contemporary journals and newspapers, policy papers, published congress protocols, memoirs, and secondary works, Soviet and Western. This is all territory quite familiar to readers of E. H. Carr's works, particularly volume 2 of *The Bolshevik Revolution* (1952).

An interesting chapter, "Scientific Rationalism in Bolshevik Ideology," offers material on the history of planning and on the debates between technocrats and democrats but does not particularly advance the author's initial hypothesis. A final substantive chapter illustrates the way in which the regime was forced to employ "shock methods" as a result of the failure of its apparatus to work in the machine-like fashion its planners envisioned. The shock methods, in turn, fragmented what was left of civil society and hastened the abandonment of the mobilization regime, signaled by the New Economic Policy of 1921.

Ultimately, the book promises more than it delivers. Adding to the current interest of historians in the process of socialist state-building, Remington emphasizes the importance of studying socialist "society-building" as well. This is an intriguing concept, but one whose definition and parameters remain unclear. We are left with a useful but not particularly novel empirical discussion of aspects of early Soviet economic organization, one whose value would have been enhanced with the inclusion of a bibliography.

DIANE KOENKER
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STEPHEN F. COHEN. *Rethinking the Soviet Experience: Politics and History since 1917*. New York: Oxford University Press. 1985. Pp. xiii, 222. $17.95

This book presents to a broad audience Stephen F. Cohen's influential ideas on change in the Soviet system. It is a historical study that aims to explain the Soviet present in the context of its past and to suggest research and political agendas for the future.

The first chapter is a stimulating and explicitly revisionist survey of Soviet studies in this country. Cohen criticizes the anti-Soviet politicization of Western studies during and after the cold war and attacks the narrow, biased "regime studies" that replaced genuine historical analysis in an era of totalitarian consensus and corresponding Whiggish views of the historical inevitability of Stalinist despotism. Cohen calls for new revisionist studies with a strong historical slant to create a "fuller and an alternative view of the entire Soviet experience" (p. 33).

Subsequent chapters (versions of which have been published before) examine the relationship between bolshevism and Stalinism, the Bukharinist alternative to Stalinism, the history of Stalinism since Stalin, and the prospects for reform in the Soviet Union today. Cohen's thesis is that Stalinism was not the inevitable continuation of original bolshevism; there were (and therefore are) alternatives to it within the Soviet system. For Cohen, the gradualist, reformist ideas of Nikolai Bukharin and the New Economic Policy of the 1920s represented an alternative to the conservative Stalinism that destroyed them. Rather than a flat, predetermined totalitarianism, Cohen sees a multifaceted Soviet historical reality characterized by constant change and by the survival over the years of a reformist, humane, anti-Stalinist idea. Bukharin is the symbol of and present-day Soviet reformers are the heirs to this social democratic tradition. Soviet politics today can only be understood in light of their history, Cohen argues, and therefore the struggle between reformers and conservatives is "simultaneously a quarrel about the Soviet past, present, and future" (p. 133).

Specialists, revisionist and otherwise, will find much to criticize among Cohen's provocative ideas. For example, he argues that the theories of totalitarianism and of an unbroken continuity from bolshevism to Stalinism are mutually inextricable. Yet it is possible to believe in one without accepting the other, as some Stalinists, Trotskyists, and modern revisionists have variously done. Further, despite his call for a hundred research flowers to bloom, Cohen proposes a rather narrow definition of "admirable" revisionist scholarship. Revisionist studies that disagree with him are traduced as "elliptical" and denied the status of "real scholarly revisionism" (p. 33).

Cohen may also overstate the case a bit when he maintains rightly that present rivalries are a function of past ones. The fate of Bukharin's reputation is surely a barometer of official and unofficial alignments. But with the exceptions of ideologists and historians it is hard to imagine many Soviet officials worrying, debating, or even thinking much about Bukharinist history on a day-to-day basis. Although today's reformers may well be proto-Bukharinists, one suspects that the struggle between conservatives and reformers may have more to do with present institutional loyalties, factional fights, and defenses of privilege, patronage, and policy than with the historiographical questions we find so interesting

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and important. Soviet historical evaluations of Bukharin are perhaps more products and indicators of the struggle than components of it.

Soviet specialists are already familiar with most of these rich and elegantly written essays, but the book serves to expose Cohen's important ideas to a broader scholarly and general public. Modern historians with limited time for books on Soviet history would do well to choose this one.

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BENJAMIN PINKUS. *The Soviet Government and the Jews, 1948–1967: A Documented Study*. New York: Cambridge University Press. 1984. Pp. xvi, 612. $59.50.

This book is an investigation of Soviet policy toward the Jewish population of the USSR in the period before the great emigration of the 1970s, and Benjamin Pinkus pursues the subject like a hungry wolf. In chapter after chapter he examines different types of evidence, first filling in background material, then summarizing the developments in his particular period, and finally pointing to the future and presenting documents to back up his points. He starts with the study of official statements and the official ideological position regarding Jewish identity. He then proceeds to the visible types of anti-Semitism, the notorious "campaigns" and purges, the trials of Jews, and policy toward Zionism. He ends with a survey of the position of the Jewish population. Seven chapters (more than half the book) discuss Jewish culture and religion, Jews in official positions, the Birobidzhan project, mention of Jews in Soviet literature, and the Oriental Jewish communities. The author includes twenty-eight useful statistical tables, 173 documents (mostly translations from widely scattered Russian and Yiddish texts), one hundred pages of footnotes, ten pages of bibliography, and a long index.

Soviet Jewry was (and still is) the third largest body of world Jewry. Although by no means the largest of the Soviet nationalities, it is among the more conspicuous and articulate ones. It deserves erudite study, and Pinkus is well equipped to carry out the task. He has had access to masses of material collected at Jerusalem and various European research institutes. Several of his chapters are truly valuable; I was particularly impressed by the exhaustive survey of references to Jews in postwar Soviet literature. Most of his documents are convincingly related to the text, and some of them are deeply moving. The editing is impeccable. No one interested in the period and subject should forego examining this work.

Yet the book is profoundly flawed as a history of the Soviet government and the Jews in the pre-1967 years because from start to finish it is an indictment. The author opens with reference to "the fact that Lenin, Stalin and their heirs denied the Jewish people recognition as a nationality" (p. 2). This is not a fact but an accusation, the more loaded because of its disguise, and here the hunt begins. In field after field he approaches his evidence from the point of view of this basic charge. Predictably, official anti-Semitism shows up everywhere. So pervasive is the inductive method that the author does not even bother us with a chapter of conclusions.

There is anti-Semitism in the USSR. No one can reasonably deny it! Nor can anyone overlook the murderous character it assumed in the late Stalin years or its "bubbling" quality under Khrushchev and Brezhnev. But does this justify Pinkus's assumptions? As he himself frequently acknowledges, not just "Lenin, Stalin and their heirs" have doubted Jewish nationality: many Soviet (and foreign) Jews do the same. In fact, over the past century the question of Jewish nationality has been a divisive problem facing Jews and their governors worldwide. That problem, not the alleged "denial," is the "basic fact" with which a study of Soviet governmental policy might best begin. It is well to recall also that anti-Semitism, while embracing its "object" (the Jews) comprehensively, is by no means so unitary in its "subject." For some Jew-haters, it is consuming; for others it can be peripheral or accidental. And this is all the more true of modern governments, which can be anti-Semitic in a thousand different ways and on a thousand different levels without necessarily being anti-Semitic in "essence."

Alas, in Pinkus's often fascinating and useful book the concept is simply lacking that the Soviet Jews may be a difficult problem that the Soviet government has tried to handle over many years in different ways and with differing success. He sees only "improvements" and "worsening" of a Soviet "policy" in which there has been "no essential change." He can see no explanation save persecution for Stalin's blocking of the "Crimean project" whether before or after 1945 or for the institution of Hungarian and Polish schools, but not Yiddish ones, in the USSR after the war. Khrushchev's antireligious campaign of the early 1960s is to him explicable substantially as anti-Semitism. The regime's publication of some strikingly favorable literary references to Jews in the mid-1960s and even its eventual decision to let Jews emigrate are treated by Pinkus as simply "shifts in policy" rather than as evidence of a fundamental puzzlement about policy, which they surely were.

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NEAR EAST

A. CHRIS ECCEL. *Egypt, Islam, and Social Change: Al-Azhar in Conflict and Accommodation.* (Islamkundliche Untersuchungen, number 81.) Berlin: Klaus Schwartz. 1984. Pp. xxiii, 611.

This ambitious, complex, and often ponderous sociological study explores secularizing trends in Egyptian society and culture, using an integrated approach akin to that of contemporary scholars of Egyptian history. It is far more than the study of al-Azhar suggested by the title. A. Chris Eccel delineates in particular the views of prominent *ulama* about religion-based issues. He concludes by questioning the future of *ulama* as religious mobilizers in the face of "increasing societal scale," "ecological confrontation," and secularization, which together have wrought a profound transformation in Egypt's Muslim institutions. To illustrate his points, Eccel includes seventy-four sociological diagrams, charts, and tables. The book has six separate indexes, thus enhancing its usefulness for researchers.

Eccel divides his study into two main parts: a history-cum-sociology of al-Azhar and an examination of attitudinal changes among *ulama* of the past century or so. In part 1 he asserts that the centralizing tendency of the state and the introduction of certification conflicted with al-Azhar's social and cultural orientation, with the effect of displacing Azharis. He draws heavily on earlier studies for illustrations of state centralization and introduction of new institutions, such as secular courts and schools, although he ignores recent studies that complement his own.

The author explains in detail how the restructuring of the regulations by al-Azhar's administrators failed to alter the quality of education, because students and teachers objected. Ultimately, to adapt to the job market, the school resorted to specialized training. This section often displays a lack of knowledge about links between al-Azhar teachers and those at state schools such as Dar al-ʿUlum. The dynamism of early twentieth-century Egyptian political, social, and intellectual history is missing. In his treatment of squabbles at al-Azhar during the 1920s and 1930s, which builds on the work of Daniel Crecelius, Eccel does incorporate the political dimension more fully. Part 1, however, emerges as an often chronologically confused amalgam of voluminous information associated with al-Azhar's history and bureaucratic change.

By far the more original and challenging section is part 2, in which Eccel explores Muslim attitudes derived from Islam. He divides them into the transcendental, the empirical, and the behavioral. The behavioral embraces not only cult and ritual but also women, entertainment and life style, commerce and economy, and internal and external controls. Eccel numbers each attitude as he discusses it. To support the existence of these attitudes and the changes that he posits, he uses an impressive array of sources. The "old" views come largely from newspapers and the writing of Muslim reformers in the past. Contemporary attitudes are more often drawn from newspapers of the mid-1970s (when the author was in Cairo), books and articles by Egyptian religious writers, and the works of colleagues. He concludes that, whereas transcendental beliefs (for example, "God exists") have changed little, the vast majority of attitudes toward behavior, especially that divorced from cult and ritual, are different today. The *ulama* may no longer have the initiative in religious mobilization, but women, schools, and family worship offer potential for continued revitalization of Islam.

Methodologically, Eccel's approach is open to challenge. He often quotes from one or two authors to establish the norm, although he presents dissenting views as well. His prodigious effort should stimulate further debate on the meaning of Islam in Egypt today.

In the main, this is an extremely dense book, with often lengthy (sixteen-line) sentences and sociological jargon. It does not fulfill all the expectations raised in the introduction. Scholars in many fields, however, will be rewarded by Eccel's synthesis of a wealth of historical and sociological data about religious education and belief. It is "must" reading for students of contemporary Islam and the history of al-Azhar.

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WILLIAM J. BURNS. *Economic Aid and American Policy toward Egypt, 1955–1981.* Foreword by HERMANN FREDERICK EILTS. Albany: State University of New York Press. 1985. Pp. xviii, 285.

Much of the recent history of the Middle East must be understood in terms of shifts in Egypt's foreign policy. And central to those shifts are the ups and downs of U.S.-Egyptian relations. William J. Burns has focused his research on one dimension of these relations: economic aid from 1955 to 1967. In doing so, he has produced a fine piece of scholarship that carefully assembles the pertinent facts, provides a clear narrative, and draws sensible conclusions that policy makers would do well to reflect on.

Burns has combed the archives and secondary works and has interviewed a large number of participants in the events he describes. As a result, he is able to shed new light on some well-studied topics. For example, the famous decision by President

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Dwight D. Eisenhower and Secretary of State John Foster Dulles first to offer Egypt funding to construct the dam at Aswan and then to withdraw the offer is treated with skill. New information on the link between the Aswan deal and secret efforts to get peace talks started between Egypt and Israel is brought to light. This is the most valuable part of the study.

Burns does an admirable job of tracing the aid relationship from 1958 to the 1967 Arab-Israeli war. The way in which domestic American politics intruded into the policy-making process is a reminder of how difficult it is to use aid for strategic purposes. Indeed, the author concludes that aid is no substitute for common political interests and that, at best, it may only help to influence the recipient's behavior on marginal issues. Although the treatment of the Sadat era is much sketchier, Burns's cautionary notes are well taken for this period as well.

The weaknesses in the book stem from the author's inability to add much on the Egyptian side of the aid relationship. Arabic-language materials might have been used more effectively. In addition, the publisher should have placed footnotes at the bottom of each page. In a work of this sort, where much of the value stems from uncovering new sources or putting pieces of a puzzle together in novel ways, the specialist wants to know what is in the footnotes. It is a distraction to have to keep turning to the end. Otherwise, this is a fine addition to the scholarly literature on U.S. policy toward the Middle East.

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ANITA SHAPIRA. *Berl: The Biography of a Socialist Zionist; Berl Katznelson, 1887–1944*. New York: Cambridge University Press. Reprint. 1984. Pp. ix, 400. $29.95.

A youthful breakaway from the traditional Jewish society of White Russia, Berl Katznelson (1887–1944) emigrated to Palestine during the pioneering spasm of socialist Zionism known as the second *aliyah*. In the interwar period he helped David Ben-Gurion and Yitzhak Tabenkin to establish several of Israel's enduring Labour party frameworks and trade union institutions and thus to bring about socialist Zionism's early hegemony. Wherein lay the source of Katznelson's immense authority? What was his particular contribution to the development of the culture and mores of the developing Jewish society of his times? Anita Shapira's achievement lies both in her subtle presentation of these questions and in her temperate search for the answers.

The issues are not entirely straightforward. Although on the inside of Zionist society, Katznelson always remained an outsider. Unlike Ben-Gurion, he did not gain influence by manipulating and dominating party or national machines; unlike Tabenkin, he never established his own base of power within the kibbutz movement. His strength, rather, lay in the force of his personality and in those elements that defy categorization except as "charismatic leadership." His was a combination of powerful thought, pungent and aphoristic literary style, and utter devotion to the total well-being of his fellow Jews. These qualities made him into the conscience of the Labour movement and a cultural moderator who attempted to put a humane face on a society whose values were coarsening and thickening.

Katznelson's tragedy is that he was not quite equal to his own goals. Always something of an intellectual vagabond, his brand of Zionist socialism was never rigidly dogmatic. He retained a deep ideological respect for Zionism's liberal heritage and a deep personal attachment to his roots in the Diaspora. These positions generated respect but could not—especially in the heated atmosphere of the 1930s and 1940s—command a lead. Moreover, at critical points Katznelson seemed to lack the toughness necessary to push his policies through. Instead, he frittered away his energies in futile involvements in the bickering feuds, brittle alliances, and fumbling attempts at cohesion that studded the uncommonly garrulous world of pre–State Labour Zionism. He flitted through several roles—fund raiser, youth leader, educator, newspaper editor, publisher—without ever attaining true greatness in any of them.

Shapira's meticulous command of internal Zionist polemics enables her to elucidate the twists and turns of all these enterprises; only occasionally, primarily on party issues, does interpretation become entangled in the documentary evidence. But her skill emerges in the portrait of how Katznelson's increasing political isolation was mirrored in an intensely personal sense of loneliness. A private person who was wholly dedicated to public life, he desperately needed love. But in this he was usually failed—by his father, his female companions, his male cronies, and, most of all, by his unborn child. In his final years the public and private spheres ultimately coalesced in the calamity of the Holocaust, about whose consequences he was uniquely presicent. Driven by the twin furies of national catastrophe and personal frailty, he ultimately died a sadly unfulfilled man.

This richly evocative biography underscores the immensity of that loss, for his own and later generations. Its translation into English (albeit in a truncated form) will allow social historians throughout the Western world to appraise a figure who is now becoming justly revered in his own country.

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AFRICA

AKINJIDE OSUNTOKUN. *Chief S. Ladoke Akintola: His Life and Times*. Totawa, N.J.: Frank Cass. 1984. Pp. xx, 191. $29.50.

To quote the burden of a paper delivered to the African Studies Association annual conference in Washington, D.C., a couple of years ago, African political biography has been characterized by its poverty—when it has existed as a genre at all. Confronted by the polarized perils of hagiography or demonology and undermined by the absence of such staples of the refined art of biography in the Western world as diaries, letters, and the memoirs of society hostesses, African biography has been the weakest aspect of the otherwise strong school of African history. Even heads of state who were in the van of recounting their struggles to reach the top have remained conspicuously silent once they have closed the doors of State House behind them. Few, of course, have survived to write their postretirement memoirs. And few African academics have chosen to write objective studies of these statesmen while the latter are still alive: that best book could all too easily become their last. As Ayandele comments in his introduction, Akinjide Osuntokun "has been more than ordinarily audacious in choosing a terrain upon which angels would fear to tread" (p. ix).

One can thus extend all the warmer a welcome to Osuntokun's short and serviceable account of the life and times of "SLA"—Chief Samuel Akintola, leader of the National Democratic party since its founding in 1964 and premier of the western region of Nigeria, who, along with his northern (but neither his eastern nor midwestern) counterpart, was assassinated in the coup d'état of January 15, 1966. (Incidentally, there would seem to be some curious confusion in the subject's nomenclature: on the dust jacket and spine he is called "S. L. A. Akintola," whereas on the title page and in the index he appears as "S. L. Akintola." He was always known popularly as "SLA." Echoes of the English mix-up of R. A. Butler and "Rab" Butler perhaps?) Clearly, there are grounds for believing that there is more to Akintola than appears in this book: one who was, on the one hand, reviled in his lifetime as a modern Afonja, the historic traitor of nineteenth-century Yorubaland because of his devastating rift with the Action Group, and, on the other, regarded as the master of Realpolitik who saved the western region from a fate worse than death by his accord with the Northern People's Congress is too significant and too controversial a political figure to be adequately disposed of in 180 pages. Osuntokun's book is, however, much nearer a first scholarly "life" than any would-be biography or profile so far published about the half-dozen regional premiers of Nigeria's First Republic or of any of the nation's other half-dozen prime ministers, generals, or presidents. It is thus a useful contribution to Nigeria's stormy political history as well as a worthwhile, though not definitive, biography of a very stormy petrel.

The book contains a few photographs and an excellent index. Despite his claim to "extensive use of oral information," Osuntokun cites only a handful of such important sources ("anonymith" on page 44 is confusing rather than convincing). The published materials quoted are standard and straightforward. Anthony Potts (p. 40) should be Douglas Pott, Mr. Justice Nbanefo (p. 153) should be Mbanefo, and C. L. Temple (p. 25, not p. 23 as in the index) was never knighted. It is perhaps the novelty of Osuntokun's encouraging example of the biographical genre rather than the presentation of much new material or fresh interpretation that calls for attention. And for such a positive contribution toward advancing the "lost cause" of African political biography many scholars will feel a sense of gratitude—that gratitude in expectation of more and even better goodies to come.

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Le Congo belge durant la Seconde Guerre Mondiale: Recueil d'études/Bijdragen over Belgisch-Congo tijdens de Tweede Wereldoorlog. Brussels: Académie Royale des Sciences d'Outre-Mer. 1983. Pp. 623.

Twentieth-century colonial Africa has not yet received adequate historical treatment despite a relative abundance of documentation. The bulk of articles and books that have appeared in the generation since independence concern a tiny part of the population: the top colonial bureaucracy, a handful of educated nationalists, and that surprisingly small section of the work force employed by transnational corporations. Although the major regions are attracting the attention of a limited number of specialists, progress is extremely slow. In part, the development of a monographic literature has been arrested by the imperatives of political independence. Countries faced with starving rural areas and paralyzed cities cannot often afford to assign their Ph.D.s to the study of a dependent past. In addition, Europeans find it embarrassing to dwell too long on the regimes that they thrust on their former subjects.

The collection under review, assembled by what was once the Belgian Academy of Colonial Sciences and funded by a foundation that bears the name (Francqui) of one of Léopold II's closest associates, marks a new stage in the historiography of colonial rule. Twenty-five years after Zairian independence, scholars from the last colonial generation and the

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first postcolonial one gathered to reassess a part of the colonial experience without the defensiveness that has long characterized metropolitan historical accounts. This is undoubtedly a result of the efforts of Jean Stengers, who has long championed a realistic approach to Belgian colonization.

The general topic of the essays surmounts previous reluctance by concentrating on the Second World War, a period on which most Belgians look back with considerable pride. Despite the fall of the mother country to the Nazis, the Congo fought on, providing the Allies with vital raw materials, including the uranium that went into the first atomic bombs. Given this record, scholars could also approach wartime issues that show Belgian rule in a less favorable light.

This new outlook is best represented by the first essay, in which Léon de St. Moulin shows that the war marked a continuation of unfavorable demographic trends among the colonial population. Through a systematic examination of colonial statistics, de St. Moulin shows that the positive vital rates, which we now take to be characteristic of the entire continent, did not appear until after the war. By the same token, Jean-Claude Willame documents the subordination of the Zairian work force to Allied demands for war production. In local studies of Stanleyville (Kisangani) and Elisabethville (Lumbumbashi), Benoît Verhaegen and Jean-Luc Vellut outline regional variants of the urban experience, showing that the inhabitants of one city were cut off from their rural cousins while others exploded with refugees from the war effort. Similarly, Gustaaf Hulstaert portrays the devastating consequences of the revival of rubber collection in Equateur Province.

Other essays reveal previously unexplored themes in the history of Belgians in Africa through biography and autobiography. Gaëtan Feltz examines the contradictory role played by one of the most complex colonial personalities, Jean Félix de Hemptinne, the archbishop of Katanga, who attacked the war effort as undermining the welfare of Africans. Frans Bontinck describes the enormous strains put on Belgian Catholic missionaries in the capital city, and with considerable charm Jacques Vanderlinden, the son of a colonial manager, describes the war from the point of view of a child.

Individually, none of the essays offers a definitive treatment of any aspect of the war. Nonetheless, the collection opens the way to a broader synthesis.

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RENÉ PÉLISSIER. *Naissance du Mozambique: Résistance et révoltes anticoloniales, 1854–1918*. Orgeval: Pélissier. 1984. Pp. 393; 407–883.

This book about the Portuguese conquest of Mozambique concentrates on "getting the facts straight." René Pélissier sets the character of his study in the introduction: "The reader will find neither system nor dogma, which can well become outmoded before the book is in print. To make up for this, the reader will find a quite substantial body of facts that each can interpret as he wants, for we do not claim that our analysis is better than others" (p. 12). Indeed, an enormous number of facts are contained in this work, which is divided into two volumes for convenience of publication rather than for conceptual reasons. Even the most ardent student of Portuguese colonialism or of the early years of Mozambican colonial history will hardly want to read the volumes from cover to cover. Yet they are an indispensable reference for understanding the various strands that made up Portuguese imperial policy and the actions of administrators and generals on the spot in Southeast Africa and for the various military campaigns over a sixty-year period in the regions that became the colony of Mozambique.

The book is divided into four sections. The first two deal with the context of conquest. First, the nature of the Portuguese presence in Southeast Africa in relation to African, Indian Ocean, and rival European powers in the 1850s is discussed. This is followed by a review of the political and economic situation in the metropole that shaped policy in Africa. Pélissier flirts with some of the ideas of the "noneconomic" school of Portuguese imperial history and with the more recent trend to view imperialism as an outgrowth of Portugal's economic weakness in relation to the other colonial powers of Western Europe. But he does not take sides, since this is not the point of his study. The main section, entitled "The Conquest," then follows. The military campaigns and African responses are described for the three major regions of the colony: the north, the Zambezi Valley, and the south. The section on the First World War starts with a chapter on the well-known Barue campaign, where Pélissier adds some details to the accounts by Allen Isaacman and Terence Ranger, and continues with a chapter on the campaign against the Germans, who invaded the northern part of the colony from German East Africa.

Those expecting a companion work to Pélissier's massive two-volume study of Portuguese conquest and African resistance in Angola may be disappointed, for this latest work is narrower in scope, though more detailed on the subjects it does cover. *Naissance du Mozambique* is primarily a military history, both from the Portuguese and from the African side. The multifaceted sweep of African nationalism contained in the volumes on Angola is not evident in the Mozambican study, which, further-

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more, concentrates more on the Portuguese side of the story. Perhaps this was wise, since, as Pélissier explains, he was unable to visit Mozambique and to work in the archives there. An extensive body of literature on African responses to colonial rule in Mozambique already exists, but the perspective of this book and its coverage of the whole country complements to some extent the studies of Isaacman, Ranger, Leroy Vail, Landeg White, Jeanne Penvenne, and others. What Pélissier sets out to do, he does very well.

Not least important in these volumes is the bibliography and the tables. The annotated bibliography of some nine hundred fifty titles is a mine of information. This is also true of the numerous charts and tables that quantify the violence of Portuguese colonialism in terms of military campaigns and economic and human costs. Of the many intriguing details, one of particular interest is that in the sixty years covered in these books, excluding World War I, Portugal annually sent to Mozambique the same number of troops as were committed to the war against Frelimo in 1983.

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J. ALTON TEMPLIN. *Ideology on a Frontier: The Theological Foundation of Afrikaner Nationalism, 1652–1910.* (Contributions in Intercultural and Comparative Studies, number 11.) Westport, Conn.: Greenwood. 1984. Pp. xiii, 360. $35.00.

South African historians have long assumed that the Afrikaners, long isolated from modernizing trends in Europe, elaborated seventeenth-century Calvinism into a modern ideology of national exclusiveness and racial domination. Few, however, have taken the trouble to argue this view systematically. To do so, one would have to distinguish three phases in the alleged role of Calvinism: (1) in preserving the racial and cultural identity of the primitive "trekboer" in the eighteenth century, (2) in animating Afrikaner nationalism in the nineteenth century, and (3) in shaping the ideology of apartheid in the twentieth. J. Alton Templin attempts to document the role of religion in the first two phases. His study is based on vast research in primary sources and secondary works, largely written in Afrikaans and Dutch and rarely read by non-Afrikaners. His is probably the most detailed argument in English for the social role of Afrikaner religion before the unification of South Africa in 1910.

Templin evidently did much of his research in the 1960s, when he published an article on the subject. Although his book refers to a few recent authors in its bibliography and footnotes, there is little evidence that he has kept abreast with the major historiographical developments of the last fifteen years. One unsettling result is that he is misinformed on the African side of white-black conflict, a failing accompanied by his use of terms like "Kaffir War" (p. 43), "tribal background" (p. xii), and "Bantu" (used to denote Africans throughout), which today give great offense.

Templin's isolation from recent historiography also leads him into serious problems of interpretation. His views fit squarely into what Martin Legassick identified in 1970 as the "frontier tradition" in South African historiography: one that interprets racial oppression in South Africa as a result of the isolation, ignorance, and embattled history of the Afrikaner. Against this interpretation Legassick argued for a more "materialist" view that would shift responsibility from religion to economics, from the frontier to the mines and industry, from the Afrikaners to the English. Since 1970 numerous revisionist scholars have developed the ideas sketched by Legassick. Templin simply bypasses this scholarship. He also downplays Afrikaners of the long-settled Cape Colony in favor of those of the interior republics. This leads him to slight the highly influential Cape Dutch Reformed Church, which had an energetic clergy, close contacts with European theology, and much influence on the Afrikaner people. This in turn permits his overemphasis on the popular, home-grown nature of Afrikaner religious ideas. The frontier emphasis also gives rise to his view that apartheid is "the logical outcome" of the conflictive history of the Afrikaner and that (modern) "economic necessities . . . are opposed to this type of barrier" (pp. 294–95). Both propositions may be true, but the host of published counterarguments cannot simply be ignored.

More seriously still, Templin takes little note of the most recent scholarship on his central queston, the relationship between Calvinism and Afrikaner nationalist ideology. For example, the highly relevant works of Dunbar Moodie and Irving Hexham are noted in Templin's bibliography, but they are never engaged. To be fair, Templin cannot be blamed for not responding to three very recent articles by André du Toit. Du Toit has meticulously demonstrated that historians of Templin's persuasion misconstrue the vaguely providential language of frontierspeople as proof that they viewed themselves as the elect of God. Templin does little to anticipate such an attack. His method is simply to lard his narrative with numerous quotations with religious content. He does not play with alternative interpretations, distinguish themes, or set ideas in opposition to one another. Thus, most of his evidence could be used to support contradictory conclusions in a debate that has now become many-sided, and almost none of it is proof against the radical skepticism of someone like du Toit. In sum, Templin has compiled a mass of useful evidence; he

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has, however, failed to restate the traditional frontier-Calvinist view of Afrikaner politics in a way likely to advance the present debate.

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ASIA AND THE FAR EAST

J. C. HEESTERMAN. *The Inner Conflict of Tradition: Essays in Indian Ritual, Kingship, and Society*. Chicago: University of Chicago Press. 1985. Pp. x, 255. Cloth $32.00, paper $14.95.

ROMILA THAPAR. *From Lineage to State: Social Formations in the Mid-First Millennium B.C. in the Ganga Valley*. New York: Oxford University Press. 1984. Pp. 189. $24.95.

Both these works address common themes: the social, economic, religious, and political forces that shaped the form and content of institutions in early Indian history from ca. 1500 B.C. to A.D. 500. J. C. Heesterman's work is a collection of thirteen essays that were published in scholarly journals or presented at conferences. They are unified through the author's explorations in what he calls the "inner conflict" between atemporal order (transcendent order of the ritual and idealized *dharma*) and the demands of the mundane world of political power symbolized by the institution of kingship. Essentially, it involves the roles of the Brahman, "attached exclusively to the order of Vedic ritualism"; the *Rājanya*-Kshatriya, who seeks transcendental legitimation of his mundane authority; and the Brahman-Renouncer, called on to "play his part in the affairs of the world" (pp. 3, 43–44).

Heesterman analyzes the social and political symbolism of the Vedic *Śtraut* ritual in several essays. The problems of political power are discussed in the essays on Kautilya and the ancient Indian state and power, priesthood and authority. The last two essays deal with the role of caste and village in Indian society, and caste and karma in Max Weber's *Analysis of Caste*. "Indian tradition" says Heesterman, makes no serious effort toward solving its inner conflict for the king and the Brahman "stand at opposite ends of each other, and there is no mediating priesthood. The dilemma is insoluble, as is the dilemma of the Dharma" (p. 7). Heesterman questions the validity of the twin concepts of Western expansion and Indian reaction. The central problem, he argues, both for the Mughals and the British, was not so much of conquest but "the extraction and management of the agrarian surplus that could be controlled" and coping with the "pervasiveness of the conflict of the inner frontier." The paper finally discusses Gandhi's role in his attempted "reorganization of Indian society on the universalistic basis of the renunciatory ideal" (pp. 162–63, 197).

To do justice to the richness of the material and insights marshaled by Heesterman in his essay is difficult. Some generalizations on the roles of the Brahman and *dharma*, as well as the role of Vedic ritual, appear to be sweeping, for the evidence from the Jain and Buddhist literatures reveals that reality was, in many instances, significantly different. But the essays are stimulating and rewarding, and their collection is a welcome addition to the literature.

Romila Thapar's work addresses the subject of social formations (and transformations) in the Ganga Valley in the mid-first millennium B.C. The first chapter examines theories of "the Asiatic mode of production" and "Oriental despotism," which, she points out, are inadequate for our understanding of state formation in ancient India. She prefers the theory of lineage as "a corporate group of unilineal kin with a formalized system of authority" (pp. 4–5, 10) as more helpful. In chapter 2 she discusses the nature and form of this lineage society, its economic underpinnings and social evolution from *Varna* to *Jāti*. She skillfully examines evidence from Vedic and Buddhist literatures, as well as archaeology, to support her thesis.

The next chapter is the core of the book. The transition from the western Ganga locale to the new eastern Ganga heartland, where the first centralized state emerged, saw the breakdown of the lineage society and its replacement by quasi-caste formations and an increased surplus, trade, and urbanism.

Chapter 4 discusses ideologies of the state, Brahmanical and Buddhist (as reflected in the *Arthaśāstra*, *Dharmaśāstras* epics, and *Puranas*), the Buddhist canonical and noncanonical literature, and archaeological evidence. The last chapter summarizes the author's conclusions on factors underlying "a process of change from social formations which may broadly be classified as lineage systems to those dominated by a state system" (p. 173).

Thapar's work, painstakingly researched and ably argued, breaks new ground for the "inner" history of ancient India. Like Heesterman, Thapar also raises a nagging question about the actual correspondence between what the texts say and the way things happened in reality. Thapar uses the Buddhist evidence to a much greater extent than Heesterman, but the question still persists. This in no way detracts from the important contributions made by these two authors to our understanding of aspects of the cultural history of ancient India.

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KONRAD SPECKER. *Weber im Wettbewerb: Das Schicksal des südindischen Textilhandwerks im 19. Jahrhundert.*

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Foreword by DIETMAR ROTHERMUND. (Beiträge zur Kolonial- und Überseegeschichte, number 28.) Wiesbaden: Steiner. 1984. Pp. xii, 302.

The optimists' view of the fate of Indian handloom weavers in the nineteenth century has been based mainly on studies of western India. The pessimists have taken their evidence mainly from Bengal. Handloom weaving in south India was therefore a suitable subject for this doctoral thesis. Konrad Specker, who based his work on quantitative data, arrives at a middling position in the controversy. Since the book has a preface by Dietmar Rothermund, it is not surprising that the literary presentation of the findings tends to have a pessimistic complexion. And, since Morris D. Morris has the most established reputation in this field in the West, it is not surprising that a simplified and rather selective summary of his optimistic view is given and singled out for criticism.

The number of handlooms per capita may have risen slightly in south India during the nineteenth century. This masked a process of deindustrialization, because of structural change in the industry, with a considerable relative growth of the sector producing coarse goods, in which there was less investment of commerical capital. One may also assume an increasing reliance on agricultural earnings by part-time weavers, but it is not clear that this constituted, in the language of Indian nationalist historians, "re-ruralization."

The debate about "proto-industrialization" has drawn attention to the well-known fact that the growth of modern industry in the world econmy has led to the deindustrialization of many regions of rural cottage weaving such as Brittany and Gloucestershire. It is thus no longer possible for a serious historian to suppose that, when this happened in a colony, it was purely a result of malignant colonialism. It is well known that the East India Company imposed unfair hardships on Indian weavers after trade was freed between India and Britain by the abolition of the latter's monopoly in 1813. The company maintained internal taxes imposed by previous Indian governments and enforced them with the strict, impartial fiscality required by Cornwallis's administrative reforms. Specker provides a precise assessment of this as a contributing factor in the process of deindustrialization in the first half of the nineteenth century.

The struggle to derive historical generalizations from defective statistics is fascinating for those actively engaged in it but makes tedious reading for others. Happily, Specker shows the diversity of response in various districts of south India to changing textile markets and especially to the availability of imported factory-spun yarn. A full explanation of such differences would require study of local institutions and mentalities, but comparative quantitative study may be a necessary step toward understanding what happened in particular cases.

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JOHN B. HENDERSON. *The Development and Decline of Chinese Cosmology*. (Neo-Confucian Studies.) New York: Columbia University Press. 1984. Pp. xv, 331. $32.50.

This is a comprehensive and sophisticated study of "correlative thought" in traditional China—thought that assumes that "systematic proportional relationships exist among such diverse realms as the astronomical heavens, the human body, musical scales, political structures and celestial beings" (p. 203). John B. Henderson relies on a wide variety of Chinese, Japanese, and Western sources to develop multicausal explanations for the rise of correlative cosmology in the third century B.C., the changes in its form over the centuries, and its demise in the most sophisticated intellectual circles of the seventeenth century. Throughout he addresses evidence counter to his theme, compares Chinese cosmology to European thought, and examines the cultural manifestations of cosmology, including its contemporary residues. The book provides a solid historical foundation for our understanding of the Chinese world view. Henderson's most distinctive contribution is his delineation of the collapse of balanced symmetry in the thought of seventeenth-century Chinese intellectuals, for whom irregularities and anomalies became principles of explanation.

Of course, any such wide-ranging study is bound to have problems; one of Henderson's is a mild tendency to underemphasize the skeptical tradition in Chinese philosphy. Since the influence of this critical and agnostic approach has long been overestimated, a corrective is necessary but not one that fails to mention Hsün-tzu, the third-century B.C. father of Chinese skepticism. Hsün-tzu redefined "heaven" as the order of nature, which is incapable of intentional response to human actions. Hsün-tzu's philosophy is based on a critical epistemology that goes back to the later Mohists, also not mentioned in this book. Another skeptic who deserves better treatment is Huan T'an (43 B.C.–A.D. 28), who lost his life through oppostion to royal divination by portents. (On this see Timotheus Pokora's *Hsin-lun (New Treatise and Other Writings) by Huan t'an.*)

One is also puzzled by the lack of any reference to T'ang Buddist thought; on page 103 Henderson jumps from the third to the eleventh century. During those eight centuries the best philosophy in China was done by Buddhists, whose views of causation and emptiness had powerful cosmological

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implications. There is enough Western material on the T'ien-t'ai and Hua-yen schools to permit at least a mention of their theories of the "absolute mind" as the substance of all phenomena and of existence as a network of relationships. The reader deserves better than a few passing references to Zen *kōans*!

The author makes several references to the influence of seventeenth-century European scientific thought on Chinese intellectuals, mediated by the Jesuits, a topic that deserves further study. It appears that the weakening of symmetrical cosmology in China during this period was owed in part to diffusion, an important conclusion that Henderson and others have so far been unwilling to draw.

The book is nicely presented, although in my copy the type bleeds through from page 176 to page 206.

Every library at all interested in intellectual history should have a copy of this pioneering book. It not only elucidates the Chinese situation but also provides rich material for comparison.

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PHILIP C. C. HUANG. *The Peasant Economy and Social Change in North China*. Stanford: Stanford University Press. 1985. Pp. xi, 369. $38.50.

This well-written study presents a new interpretation of how rural north China evolved over three centuries and experienced a crisis in the second quarter of the twentieth century. Using Japanese rural surveys and new archival information, Philip C. C. Huang argues that the region experienced gradual commercialization in the eighteenth century that accelerated after the 1890s. But such commercialization never generated a Green Revolution; instead, the region underwent involution. Managerial landlords still hired wage labor and never accumulated sufficient capital to produce qualitative breakthroughs in agricultural productivity. A proletariat did not emerge, because most farmers owned their pygmy plots and found employment off their farms. Twentieth-century north China, neither capitalist nor premodern, was a paradox.

But managerial landlords and small farms increasingly displayed different socioeconomic behavior. By the 1920s population pressure and more exploitative behavior by large farms created new conditions: more large farms began "bumping tenants"; more tenants paid high, fixed rents; more wage laborers worked without the "ceremonial niceties" employers formerly offered. Furthermore, new elements tilted this regional economy toward crisis: officials imposed more taxes on villages, the quality of village leadership declined, community solidarity deteriorated, and "poor peasants" became a source for revolutionaries to tap.

Huang's interpretations stand or fall on the historical veracity of his concepts "involution," "peasant differentiation," and "commercialization." He defines involution as a process in which farmers apply "more labor than was optimally necessary" at the "cost of a certain unavoidable slackening of effort" (p. 155). But Huang presents no evidence that involution had worsened. The only table cited (p. 188) is controverted by Loren Brandt's unpublished findings for that same village in 1938–39 as well as from other Mantetsu surveyed villages that show poor farmers instead earned positive "net profits" and had not applied more labor than was optimally necessary. As for peasant differentiation, Huang's claim that large farms behaved differently than small farms is not borne out by the Mantetsu data. Again according to Brandt, farms smaller and larger than 30 mu. allocated their resources efficiently and used labor only until its marginal value product was equal to the market wage. As for market exploitation, Ramon H. Myers's *The Peasant Economy* (1970) shows that in north China, under accelerated commercialization, the labor, land, and capital markets were highly competitive without monopsony and monopoly power. John L. Buck and Myers demonstrated that all classes of farms benefited from commercialization initiated by foreign trade after the 1880s, and Huang never refutes that interpretation.

Why should managerial landlords have accumulated more capital to initiate a Green Revolution? That did not happen elsewhere in Asia until the state took the lead with new investments and restructured property rights. Then, both small and large farms adopted high-yield seed varieties and new capital inputs. By ignoring the origins of Asia's Green Revolution, Huang misleads his readers.

What really caused the agrarian crisis in north China after 1920? Myers argued that the greater frequency of external shocks like warlord policies, weather calamities, and Japanese aggression led to higher taxes and greatly disrupted commercialization, which was primarily responsible for rising rural incomes in the first place. Huang neither refutes this thesis nor acknowledges that other scholars have presented it.

Previous scholarship has explicated a more convincing interpretation of north China's evolving rural order: the village economy increasingly accommodated more population because of greater commercialization and the activities of rational, economizing farmers who did not exploit each other in the marketplace. The system worked so well in terms of off-setting commodity and resource scarcities that innovations were discouraged. Had China been governed by an elite that favored new invest-

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ments in agriculture, north China would have incurred a Green Revolution. But in 1911 the state dissolved, and more frequent shocks to that rural order eventually brought it to a state of acute crisis in the 1930s. Readers should consider this interpretation when they read Huang's study.

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MADELEINE ZELIN. *The Magistrate's Tael: Rationalizing Fiscal Reform in Eighteenth-Century Ch'ing China.* Berkeley and Los Angeles: University of California Press. 1984. Pp. xviii, 385. $35.00.

This book concentrates on the legalization of the meltage fee, a salient aspect of the tax reform in eighteenth-century China. The meltage fee was an illegal surcharge on the land and labor-service levies, collected by the local government as an informal funding system. Eventually, the Yung-cheng emperor legalized its collection at relatively lower rates and set guidelines for its management. The first two chapters of this work by Madeleine Zelin discuss the background of the meltage fee. The remaining five chapters deal with the formulation of policy to legalize the meltage fee and its implementation, problems, and final breakdown.

Zelin's material is chiefly from the Ch'ing archival centers: the National Palace Museum in Taiwan and the Number One Historical Archives in Peking. Her approach is both intensive and systematic. In discussing the execution of the policy, for instance, she provides a province-by-province analysis. She extends her coverage to the early nineteenth century and broadens her study by including the customary fees and gifts, another informal funding method related to the meltage fee. Many of her analyses are accompanied by tables, each of which is packed with statistical figures sifted from numerous memorials. This substantive analysis, together with a broad coverage, reinforces her theme that the traditional Chinese state was capable of innovative reforms.

The book is disappointing in several areas. Zelin tends to oversell her interpretations. She relates the legalization of the meltage fee to the development of a modern state and its breakdown to the fall of the Ch'ing dynasty (pp. xii, 304, 308). Without the Yung-cheng emperor's "stengthening of the palace-memorial system," the legalization of the meltage fee "could never have taken place" (p. 167). Although interesting, these explanations are questionable. Some interpretations are incorrect. For example, tax evasion and arrears of the Chinese in the Kiangnan region resulted mainly from corruption, heavy taxation, and the acquisitiveness of certain taxpayers, not from "southern-gentry opposition to Manchu rule" (p. 128). The tax the K'ang-hsi emperor fixed on a permanent basis in 1711 was the labor-service levy, not the "taxes" in general (p. 273).

The book contains countless romanized Chinese terms, many of which are used as regular words in the text (*kuan-chia, wo-cho* [pp. 53, 230]) and have various ramifications. Quite a few Chinese terms are inaccurate (*pang-tieh, fu-liang* [pp. 138, 161]); some are improperly or not at all explained (*tan-ku* [*sic*], *ch'ing-ts'e* [pp. 77, 199]; *hsieh-ch'a, wen* [pp. 231, 289]). Most romanized terms are excluded from the index and some even from the glossary (*feng-kung, ling-pi* [pp. 87, 176]). Actually, a majority of the romanized words are expendable.

The bibliography and notes could be further streamlined. Perhaps because of her emphasis on the primary sources, Zelin neglects some secondary works closely connected with her subject. The article by Ching-ping Tang details the "nourishing virtue silver" (*New Asia Journal*, 10 [July 1973]: 249–336). My *Autocracy at Work* (1974) has 12 pages on the meltage fee but was not consulted. A spot check of the bibliography indicates that a citation reference (p. 313 n. 26) is missing. Her citation of memorials only includes the memorialist's name and the date of the memorial. Without the page citation, it is hard to identify a specific document because many officials submitted several memorials the same day. Her dating practice for references from the Ch'ing *Veritable Records* is inconsistent (pp. 314 n. 35, 327 n. 14). Finally, table 4.1 needs documentation, and the figures for Chihli and Kiangsu provinces in table 4.2 should be reexamined. Names such as Shih Yü-lin, Yü Chu, and O-erh-ta, to mention only a few, should be Hao Yü-lin, Maiju (Mai-chu), and Omida (o-mi-ta). Nevertheless, this is the first book-length study of the topic. The topic is important; so is the study.

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S. A. M. ADSHEAD. *Province and Politics in Late Imperial China: Viceregal Government in Szechwan, 1898–1911.* (Scandinavian Institute of Asian Studies Monograph Series, number 50.) London: Curzon. 1984. Pp. 139.

S. A. M. Adshead opens this compact study with the assertion that the locus of political power in the late Qing was the governor-general, a figure whose influence eclipsed even the throne's. The author's concern, though, is neither with the debates on regionalism nor with the mechanics of provincial government. Adshead's argument is that the governors-general weakened their dominant position by attempting to use traditional means for

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modern ends and in the process strengthened Szechwan's informal hierarchies, variously referred to by Adshead as "the country" and "society."

Adshead traces the careers of Szechwan's governors-general between 1898 and 1911 in terms of how each contributed to the destablization of the pre-1898 equilibrium between center, province, and foreigner and how the province proceeded from the "best in [imperial] China" to the "worst of a half-modern Republic." He moves quickly through the passive K'uei-chun and the "first disruptive modernizer," Ts'en Ch'un-hsuan, to concentrate on the cautiously activist Hsi-liang and the centralizing Chao brothers, Erh-hsun and Erh-feng. The successive reforms of the salt administration, the reorganization of the Tibetan marches by Hsi-liang, and the anti-opium campaigns of Hsi-liang and the Chao brothers are each given their due, but, because Adshead does not provide an early and clear working definition of "modern," the linkage between his narrative and thesis is not always clear.

Other recurring problems mar this useful work. Adshead tends to treat fairly common circumstances as unique to Szechwan. Other provinces were also characterized by dual hierarchies, one formal and one informal. A comparative perspective, even a brief one, is needed. Similarly, assertions about the "mass" nature of the opium campaigns need more substantiation. Finally, Adshead distracts the reader from his argument with rubrics and epithets that often annoy more than enlighten. Local elites, for instance, may parallel Creole elites, but provincial officials are hardly carpetbaggers. Why characterize organized Christians as "Mafia," the Kuomintang as a "bright urban machine," and Chinese communism as a "half-urban half-rural monstrosity"?

Still, Adshead's work, particularly the concluding chapter, is replete with suggestive, cogent ideas. Educational reform shifted the political balance horizontally, not vertically, from state to society by freeing informal elites from dependence on the imperial system and turning them toward alternative sources of social legitimation such as economic development. But, Adshead concludes, it was not the bourgeoisie of Chungking that seized control when the governors-general finally abandoned their traditional means and turned to modern ones, such as political participation; it was the old ruling class of Chungking.

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HUNGDAH CHIU, editor. *China: Seventy Years after the 1911 Hsin-Hai Revolution*. Assisted by SHAO-CHUAN LENG. Charlottesville: University Press of Virginia. 1984. Pp. x, 601. $20.00.

The editors of this volume describe it as a systematic comparison of two parts of China, the mainland and Taiwan, that have been separated since 1949. In fact, while the book is several different things, it is far from a systematic comparison. It consists of individual essays by eighteen different authors, fifteen of whom treat Taiwan and the mainland separately in the categories of historical background, political development, economic development, legal development, cultural and educational development, social development, and "China and the World." Three concluding essays attempt to tie those separate strands together. Most of the authors concentrate on the post-1949 period, but the overall perspective goes back to 1911; only one author (Ramon H. Myers) takes seriously into account the important historical fact that Taiwan and the mainland have been separated from each other since 1895.

The format of the book is not conducive to systematic comparison. The historical essays do not make clear the very different histories of the two parts of China and utterly fail to establish an analytical basis for the comparison that is supposed to follow. Only the concluding chapter points out that comparing two such vastly different entities is problematical: one was an island of six million people in 1945, just emerging from fifty years as a Japanese colony; the other, the largest polity on earth just emerging from decades of civil war and invasion. These and other problems of comparative analysis are never systematically confronted. No conceptual framework is provided, and no common body of questions is addressed by authors ostensibly dealing with the same topic. (The two writers on "social development," for example, treat their subject in ways that have almost nothing in common.)

Countless potentially fruitful areas of comparative analysis are totally ignored. Historians and social scientists would dearly love to have a systematic comparison of Taiwan and mainland China that focuses on questions raised by scholars in recent years concerning Chinese political culture, effects of industrialization on the Chinese family, the changing character of Chinese elites and their role in modernization, the role of the state and of foreign capital and expertise in economic development, changing state-party relations (especially in one-party systems), party-army relations, factionalism in Chinese politics, leadership cults, ideological orthodoxy and censorship, the changing roles of youth and women, and much more. No such comparative analysis takes place in this book. Women are scarcely mentioned. (There are also no women authors among the eighteen contributors. This is a considerable surprise, since women scholars have for many years done much of the best work in Chinese studies.) Insofar as the book exhibits systematic compar-

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ative analysis, it is limited for the most part to certain questions about economic development, especially those that show Taiwan in the best possible light.

The theme of the book is that Taiwan is a success and mainland China a failure. The authors clearly believe that Taiwan (recognized in this book as the "Republic of China" and not simply as a part of China) has received insufficient attention and that the Guomindang's reputation is unfairly tarnished. They do their best to redress the balance. The result is a one-sided book, most richly detailed in those areas where Taiwan's case is strongest and the mainland's weakest, sympathetic with respect to the Guomindang's problems but unfailingly harsh in its judgments of the Chinese Communists. Although the book contains a considerable body of well-informed scholarship, and one essay (by Richard Curt Kraus on the social consequences of poverty in the People's Republic of China) is particularly stimulating, the simplistic use of a success-failure framework of analysis and the frequent excessive claims for Taiwan mar what could have been a valuable contribution to knowledge. Taiwan's successes are of great interest to students of development and should be accurately assessed and widely discussed. Comparisons between mainland China and Taiwan, however, require far more sophisticated handling and less partisanship than is evident in this book.

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RICHARD T. CHANG. *The Justice of the Western Consular Courts in Nineteenth-Century Japan.* (Contributions in Intercultural and Comparative Studies, number 10.) Westport, Conn.: Greenwood. 1984. Pp. xiv, 183.

The administration of justice by Western extraterritorial courts in Japan in the later nineteenth century is a neglected topic. Richard T. Chang's short monograph provides an assessment based on evidence from four notorious episodes in which Japanese contemporaries and historians alleged that gross bias worked against Japanese nationals and interests. This is followed by a brief chapter looking broadly at the problems of evaluating mixed cases. The author states the character of each episode, investigates the evidence from British, American, and Japanese sources, provides a comparison with other relevant cases, and pronounces his own conclusion. At the start Chang explains the difficulties presented by the absence of sources: only British and American records reflect Western views satisfactorily, since the records of the other Western powers exercising extraterritorial rights have long since been destroyed. It is appropriate to focus on disputes involving Britain, because it played the dominant Occidental role in the development of Japan from the 1860s to the 1890s.

The first episode examined is the King case (1875), in which an Englishman accused of raping a thirteen-year-old Japanese girl was found guilty and sentenced to six months' imprisonment. In this emotive issue the British community and Japanese opinion were equally upset; the former questioned the guilt of the accused and the latter the leniency of the sentence. Chang concludes that the verdict was correct and the sentence was a mild one when compared with sentences in contemporary rape cases in England.

The second dispute, the Hartley case (1878), concerned the importation of opium and was particularly controversial, because the case for the defense rested on the prohibition against importing opium except for medicinal purposes. Furthermore, the dispute involved a Japanese attempt to undermine extraterritoriality, as emphasised by Sir Harry Parkes (p. 52). Chang conludes that the Japanese position was less convincing because Japan was seeking to ignore the treaty agreements stipulating the role of the British consular authorities.

The third episode, the Drake case (1886), concerned a sentence of three months' imprisonment passed on a British merchant captain who had failed to make a serious attempt to save the lives of twenty-five Japanese passengers when his ship struck a rock. Here again Chang maintains that Japanese criticisms of bias were invalid: Drake was found guilty after trial by jury, and the presiding consul admonished him for failing to demonstrate the high standard expected of British merchant captains.

The fourth dispute, the *Chishima* case (1892–95), concerned the sinking of a Japanese torpedo boat when it collided with a British vessel. Part of the controversy resulted from the Meiji emperor becoming the plaintiff on the advice of the British legal adviser to the Japanese government. The case was settled out of court owing to Japanese feeling that the emperor had been cited mistakenly and because of the desire not to affect relations with London adversely when Britain had agreed to the end of extraterritoriality.

Chang concludes that, in the main, Western consular courts acted fairly. His estimate is that virtually all of the thirty-five hundred mixed cases (involving British, Americans, and Japanese) went unchallenged; probably less than 1 percent were unfairly adjudicated. This is a careful revisionist study, which extends our understanding of the Western relationship with Japan in the early Meiji era.

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SANDRA C. TAYLOR. *Advocate of Understanding: Sidney Gulick and the Search for Peace with Japan.* Kent, Ohio: Kent State University Press. Pp. xiv, 260. $25.00.

This book paints a warmly sympathetic portrait of a minor figure in the history of American–East Asian relations. Sidney L. Gulick actually had four overlapping careers: missionary to Japan, 1897–1913; champion of justice in immigration legislation, 1906–24; lobbyist and publicist for Christian internationalism, 1914–34; and spokesman for understanding Japan from 1934 to his death in 1945. He was, Sandra C. Taylor argues, a "loser" in each of these roles, but his career as a whole is rich with meaning for American–East Asian relations during the first half of this century.

Taylor draws out this meaning with grace and skill. She provides a superb analysis of his early years, pointing out how personality, self-doubt, and ignorance of Japan ill-prepared him for his first career. Although her account of his years in Japan reveals little of the larger society in which he lived and offers no Japanese evaluations of Gulick, it does present an engrossing and accurate description of the transformation of evangelist into theologian into publicist.

Taylor subtly weaves together the details of Gulick's careers as immigration legislation reformer and peace advocate with the larger history of the immigration and peace reform movements. Although experts will find much that is familiar in her account, they can learn a great deal from her description and analysis of military intelligence surveillance of Gulick. Taylor shows that charges that Gulick was a Japanese agent were baseless, but her account of how his foes (including a leading American missionary bishop in China) used those allegations provides fresh insights into the politics of American–East Asian relations. Gulick's experiences in the 1920s prefigures those of State Department "China hands" three decades later.

Taylor nicely explains why Gulick's knowledge of Japan did not assure influence on policy. He was "occasionally inconsistent, sometimes contradictory, and irrepressibly idealistic" (p. xii) in his writings on American-Japanese relations. Although respected in Japan, he lacked close and continuing ties with government officials there or in Washington. He also divided his efforts among a wide variety of causes, much like reformers in mid-nineteenth-century America, thus diluting their force. Not least, Gulick too often read his own notions of Christian progressivism into Tokyo's actions in a way that contradicted Japan's increasingly nationalistic and imperialistic behavior.

This book deserves a wide readership. It presents a warmly human picture of the Japan missionary experience not to be found elsewhere. It treats a wide variety of issues pertinent to early twentieth-century American diplomacy in a way certain to prove appealing and instructive to undergraduate readers. In addition, the book shows contemporary "advocates of understanding" between the United States and culturally different nations the antecendents of problems that continue to confront them.

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JAMES WILLIAM MORLEY, editor. *Japan Erupts: The London Naval Conference and the Manchurian Incident, 1928–1932; Selected Translations from* Taiheiyō sensō e no michi: Kaisen gaikō shi. (Japan's Road to the Pacific War; Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1984. Pp. xi, 410. $35.00.

Japan's path to Pearl Harbor began on September 18, 1931. On that day Colonels Ishiwara Kanji and Itagaki Seishirō successfully staged an incident that eventually led to the conquest of Manchuria by the Kwantung army. Step by step, the two colonels were able to carry out their schemes without being restrained by their superior officers or by Tokyo. This has led historians both in the West and in Japan to argue rather convincingly the importance of the roles played by the mid-echelon officers in determining Japan's national policy in the prewar years.

The present volume, the fourth in a series of translations of *The Road to The Pacific War*, contains essays by three prominent Japanese historians and does not differ materially in the interpretation of the roles of staff officers. It does, however, provide rich background material showing how party politicians, elder statesmen, and the military high command came to terms with these mischief-making staff officers.

The first essay is "The London Naval Treaty, 1930" by Kobayashi Tatsuo (translated with an introduction by Arthur Tiedmann). Would the limitation of naval strength agreed on in London constitute an infringement of the right of military command as stated in Article 11 of the Meiji Constitution? Historians generally agree that the introduction of this issue by the navy created the thorniest problem for the Hamaguchi cabinet. According to Kobayashi, however, it was less of a command issue than a political one. Admiral Katō Kanji, the chief of the Navy General Staff, was not at first concerned with Article 11. He embraced this issue only after it was presented to him by Mori Kaku, the party secretary for the opposition *Seiyūkai*. The latter, with only 173 seats in the lower house, could not effectively challenge the 271 absolute majority enjoyed by the *Minseitō*. By raising this constitutional issue, the *Seiyūkai* succeeded in embarrassing the

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Minseitō and set in motion a process that weakened the position of the party government irreparably.

"The Manchurian Incident, 1931" by Seki Hiroharu (translated by Marius Jansen) focuses narrowly on the main players and on their motives. It has a sense of immediacy, if not a broad perspective. Thus, one could almost sympathize with Ishiwara when he refused to share the action plan with his co-conspirator, Major Hanaya Tadashi. Seki capably describes the public relations effort, through the Manchurian Youth League, to bring their case to Japan proper.

Shimada Toshihiko provides the third and final essay, "The Extension of Hostilities, 1931–1932" (translated by Akira Iriye). It contains six episodes, including the spread of the conflict to Shanghai, that can be read jointly or separately. To borrow Mao Zedong's words, the Kwantung army went "two steps forward, one step back" in conquering Manchuria. Tokyo rejected and then accommodated. The Kwantung army complied and then created a fait accompli from which there could be no escape.

Reading the three essays together in this well-rendered translation, one is struck by the difference in the style and approach of Japanese scholars. If they are not willing to generalize and analyze, they compensate ever so richly by their skillful use of primary sources and by their creation of a sense of participation in the unfolding events. This book is an important addition to our knowledge of the Second World War, of the decision-making process in another society, and of Japanese scholarship.

History is contemporaneous, and abundant lessons can be gleaned from this volume for today's world. The Shimada essay describes the ease with which international incidents can be manufactured and then used as justification for military expansion. The Kobayashi essay shows an inherent danger that exists in any country's overreliance on an arms limitation agreement. When Japan's demands for auxiliary vessels were not fully met, one of the staff officers was heard to say that it did not really matter, because air power would be the thing of the future. And it was the air power that proved decisive at Pearl Harbor.

DAVID J. LU
Bucknell University

YOUNGNOK KOO and SUNG-JOO HAN, editors. *The Foreign Policy of the Republic of Korea.* (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1985. Pp. xiv, 306. $25.00.

This book consists of nine chapters written for the volume and two chapters previously published and partially revised and updated. The contributing authors, all leading Korean scholars in South Korea and the U.S., have written extensively on their topics. This volume is unique because it presents a Korean perspective on the subject. It describes, analyzes, and explains the substance and nature of the foreign policy of the Republic of Korea (ROK) since the end of the Korean War, with emphasis on the foreign policy of the ROK under Park Chung Hee (1961–79). "Intended as a general description of the foreign policy of the ROK, and not as a theoretical treatment of the subject" (pp. x–xi), the book is well organized, clearly written, and scholarly.

The editors, Youngnok Koo and Sung-joo Han, begin with a brief chapter on the historical background of the ROK's foreign relations. Nine specific studies follow, explaining the foreign policy–making process, major policy areas, and global policies of the ROK: foreign policy decision making written by Koo, national security policy by Young Hoon Kang, unification policy by B. C. Koh, foreign economic policies and economic development by Eul Yong Park, policy toward the U.S. by Han, policy toward Japan by Bae Ho Hahn, policies toward China and the Soviet Union by Ilpyong J. Kim, South Korea's Third World policy by Jae Kyu Park, and Korea and the United Nations by Chi Young Park. The volume concludes with the editors' overall evaluation of Chun Doo Hwan's foreign policy (1981–83) and the future prospects of South Korea's foreign policy.

Most of the contributors have written well-documented and reasonable analyses of South Korea's foreign policy from a South Korean perspective. It is regrettable that each author's invaluable arguments and insights contained in this volume cannot be discussed in detail because of space limitations, but several patterns of South Korea's foreign policy drawn from this volume are worth mentioning. First, the June 23, 1973, Declaration of Peace and Unification by President Park Chung Hee provided the essential principles and guidelines of the ROK's foreign policy under which Park's foreign policy decisions were made and implemented (pp. 36–37). Second, the ROK has pursued a policy of peaceful coexistence with North Korea and other communist states to reduce tensions and establish a durable peace on the Korean peninsula. Third, the ROK has developed a multilateral diplomacy to achieve national security and economic modernization. Fourth, the ROK has pursued a policy of peaceful unification through inter-Korean dialogue and cooperation.

The editors make three predictions concerning South Korea's foreign policy patterns of behavior in the coming years: (1) the ROK's security will continue to have the highest priority; (2) the ROK government will continue to emphasize economic relations with advanced industrial nations and the

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Third World; and (3) domestic political, social, and economic stability will be a *sine qua non* for achieving ROK foreign policy goals.

Koo and Han edited this volume superbly, and, overall, they make a valuable contribution to our understanding of South Korea's foreign policy under Park Chung Hee. This book is "must" reading, not merely for students of Korean affairs but also for those wishing to know about the history of foreign relations of this newly industrializing country, a country that is expected to play a significant role in world affairs in the coming decade.

TAE-HWAN KWAK
Eastern Kentucky University

DAVID K. WYATT. *Thailand: A Short History*. New Haven: Yale University Press. 1984. Pp. xviii, 351. $27.50.

In range and scope this would seem to be the most ambitious history of the Thais ever published. It moves from theories on the origin of Tai-speaking peoples several millennia ago right up to the 1980s. Much of David K. Wyatt's concern is political; the book deals in the main with state formation, internal struggles for power, warfare, and the Thai state's international relations.

The first chapters are impressive. Wyatt guides the reader through a maze of hypotheses concerning the earliest history of the Tai-speaking peoples. After the first one hundred pages, when the narrative has already reached the sixteenth century, this confident style, with the careful weighing of often-contradictory pieces of evidence, is abandoned. For late Ayudhyan times and after, Wyatt choses to adhere closely to the Thais' official version of their own history, even where this rests on shaky evidence.

For example, the story of King Naresuan is retold in the romantic legendary form, just as it is told to Thai schoolchildren. Wyatt even quotes Naresuan's words before this heroic figure engaged in a fight at the end of the sixteenth century: "Come forth and let us fight an elephant duel for the honor of our kingdoms" (p. 103). Such nineteenth-century fabrications do not deserve a place in a reputable history book; Portuguese and Dutch sources give a different perspective on that period.

As the book takes us toward modern times, it is apparent that, in general, the author maintains the standard view of the past as it has been promulgated by the Thai elite. The more awkward events are portrayed as temporary aberrations, and Thailand's past leaders' saving graces are emphasized. There is little evidence of a critical appraisal of late nineteenth- and twentieth-century Thai sources. Hence, Rama I is the nation's savior, not a usurper; Rama II, a poetic innovator; Rama III, the unsympathetic interlude before the triumphant reigns of Mongkut and Chulalongkorn. The measures of Chakri kings are generally shown from a charitable viewpoint. Even rather unusual events, such as Chulalongkorn's decision to marry three of his half-sisters, are presented as the Thai norm (p. 197). A quick survey of the royal genealogy would show this not to be the case.

In the last chapter the author regains his magisterial style, giving a thoughtful survey of recent events and the changing conditions of the Thai political scene. On the whole, this reviewer finds *Thailand: A Short History* an uneven work. Parts of the book are definitely authoritative, but others represent little more than the "cleaned up" version of Thai history as it is promoted by Thai nationalistic authors.

B. J. TERWIEL
Australian National University

SONG ONG SIANG. *One Hundred Years' History of the Chinese in Singapore*. Foreword by EDWIN LEE. New York: Oxford University Press. 1984. Pp. xxii, 602. $54.00.

By its first centennial in 1919, Singapore had become a multiracial, pluralistic society, to which the Chinese immigrant community contributed the bulk of the population. Song Ong Siang's chronological history of the Singapore Chinese was meant to commemorate the technical and entrepreneurial skills and the wealth and potential influence that followed the first wave of immigration to this British colonial city in 1821. In recording the lives, pursuits, and material success of many Victorian-era *Towkays* who clawed their way into the unsympathetic environment of the *Nanyang*, Song reminds us that the straits immigrants who were in positions of leadership tended to be loyal to the king and a new empire, the British. Song also intended to impress the immigrant Chinese, who had carved out a new way of life for themselves following the rise of Singapore, with the idea that their prosperity was a direct result of the mingling of Chinese and Western values and that they should face the future in the spirit of national unity in the "new empire." The book was first published in 1923, and words like "honours," "addresses," "clubs," and "landmarks in education" constantly recur in the author's rhetoric with the aim of strengthening the loyalty of the local Chinese to, and their authority and prestige with, the British government.

Reared in the Christian faith, educated at Cambridge, and comfortable with the verse of Shakespeare, Wordsworth, and Tennyson, Song straddled two cultures. The dust-jacket photograph

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shows a dignified older man faultlessly attired in waistcoat and collar festooned with decorations as a mark of service to British rule and the welfare of the Chinese community. Song's version of the history of Singapore Chinese corresponds better to the aspirations of the straits-born Chinese and reflects the genuine history of their society—their livelihood, public services, recreation, and festivities. The more than one hundred fifty vivid photographs reproduced in the book were an effective reminder to the Chinese community, and, especially, its more prominent individual members, of the value that the straits-born Chinese in particular put on British citizenship, education, and the introduction of modern values.

Song encyclopedically records more information about ordinary Chinese in Singapore than most earlier works in Chinese, but one has to search the thirty-seven-page index with care for the fragments of the life stories of "faceless" coolies who did the difficult dirty work of building and binding a nation for which the qualification was to be unskilled and the reward low wages. Song's work, though conventional in format, is not passé. Recent trends in historiography and in the study of overseas Chinese communities make Song's history today a mine of information on nineteenth-century Chinese society in Singapore for the social historian. In this new edition Edwin Lee has provided a fine biographical piece on Song as an introduction. The reprinting of this history of the straits Chinese is most welcome at a time when the reclamation of heritage in Singapore has an urgency that is narcotic.

JIM WARREN
Murdoch University

D. S. RANJIT SINGH. *Brunei, 1839–1983: The Problems of Political Survival.* New York: Oxford University Press. 1984. Pp. xii, 260. $29.95.

As the anthropologist D. E. Brown has remarked, the word "Brunei" can mean many things: a town, a river, a people, a district, a society, an empire, and the whole island of Borneo. By 1839, when D. S. Ranjit Singh's study begins, Brunei had shrunk from an empire to a petty sultanate making increasingly ineffectual claims to suzerainty over the northern coast of Borneo. In the course of the next hundred years the sultanate was reduced even further to two adjacent but detached territories, a mere 2,260 square miles in extent, clinging to what little international personality it had left solely by virtue of its status as a British protectorate. That the country enjoyed even this remnant of selfhood and was not completely swallowed up by its neighbors, Raja Brooke's Sarawak and the British North Borneo Company's Sabah (both of which, ironically, it had spawned), was largely because of the discovery of oil in commercial quantities, which made the British government think again about letting Brunei go completely under. And it is, of course, oil that today sustains the tiny independent polity (population 212,480) and not least the absolute monarchy of the state of Brunei, which in 1984, reluctantly relinquishing the yoke of colonialism, became the 159th member of the United Nations.

Singh's book is an account of all this, cast largely in the form of a study in international relations. But the author is also concerned, somewhat against his own evidence, with detailing the ingenuity and resourcefulness of Brunei's rather small ruling class in avoiding extinction by the successive forces of nineteenth-century capitalist imperialism, colonial administrative rule, the Japanese occupation, the coming of constitutional monarchy to its Malay counterparts in the peninsula, seduction by a nascent Malaysia, popular internal revolt, and the throat-clearing efforts of the retreating and decolonizing British who would have preferred to see a more democratically representative successor regime.

Partly because Singh's sources are a little limited—for the early period, he uses mainly British colonial records; for the later, mainly newspaper reports and government statements—and partly because of a certain naiveté in approach, the book seldom penetrates much beneath the surface of events, but at that level it provides a straightforward and useful account of the survival of an anachronism.

WILLIAM R. ROFF
Columbia University

DAVID JENKINS. *Suharto and His Generals: Indonesian Military Politics, 1975–1983.* (Monograph, number 64.) Ithaca: Cornell Modern Indonesian Project. 1984. Pp. xiii, 280. $12.50.

David Jenkins opens his acknowledgements with a list of names of retired army officers, well known as critics of the present Indonesian regime. He goes on, however, to thank others who are still in government ranks and who, he claims, supported his study and accepted that it should show warts and all. Earlier studies of the political role of Indonesia's armed forces have shown that senior officers in Indonesia are indeed prepared to be remarkably frank in their discussions with trusted outside observers. Whether they will be satisfied with the result in this case one cannot know, but Jenkins's picture certainly displays the warts.

The study opens with a sketch of the power structure in Suharto's Indonesia. It describes the president's effective mastery of Parliament and the People's Deliberative Assembly and the army's sys-

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tem of control exercised through its operational command structure, its parallel territorial organization (penetrating down to village level), and the appointment of retired or seconded officers to positions in the civil administrative hierarchy. This common structure is further strengthened by the overlapping and reenforcing powers of the core group of officers surounding Suharto and controlling the intelligence and security services and the Ministry of Home Affairs.

Since 1975 this all-embracing system has come under challenge not only from elements outside government circles but from within the ranks of the armed services as well. Jenkins's main purpose is to trace in detail the course of a longstanding debate about the proper role of the armed forces in Indonesian government and society. Accepting the legitimacy of the army's role as a social and political force, the debate has turned essentially on two issues. Should the army stand above other groups in society, or should it enter the political arena directly, choosing its friends and allying itself with selected groups? Secondly, how extensive a role should it play in day-to-day administrative matters? The critics, chief among whom is General A. H. Nasution, have argued that the army should belong to the people as a whole and not to any one group. It should therefore stand above the political fray. In consequence, they condemn its thoroughgoing support of the officially sponsored political organization Golkar (Functional Groups). And they have called for a much-reduced military presence in civil affairs.

Jenkins examines this challenge with care. At the end of the book he suggests that, theoretically, the debate could go either way in the future—toward bringing the army back to the barracks or toward a more authoritarian intervention. He believes in the end, however, that practical imperatives will lead the army to hold on to the powers it now possesses.

This is hardly a book for the general reader. It is densely written and assumes a good deal of background knowledge. Nevertheless, based as it is on intimate and direct contacts with the main participants in the story, it is an essential source for the last decade of Indonesian political history. It is to be regretted that there is no index (except of proper names) and no bibliography.

J. D. LEGGE
Monash University

UNITED STATES

DAVID E. SHI. *The Simple Life: Plain Living and High Thinking in American Culture*. New York: Oxford University Press. 1985. Pp. viii, 332. $19.95.

David E. Shi sets out to trace "a rich tradition of enlightened material restraint in the American experience dating back to the colonial era" (p. 3). To "bring texture" to the study of ideas and penetrate "the mystery of daily living," he proposes to examine "the simple life" at three levels: "as a sentimental ideal," "as an actual way of living" for individuals and groups, and as a subject viewed by "the larger public . . . over the years" (p. 7).

In fact, much of the book addresses fairly familiar topics in intellectual history. After a brief look at precedents for an antimaterialistic philosophy of living in Asian religions, in the classical world, and in the Judeo-Christian tradition, Shi furnishes ten workmanlike chapters on the Puritans, the colonial Quakers, the revolutionary generation, Jeffersonian Republicans, Jacksonian elites and reformers, the transcendentalists, the Mugwumps, social thinkers of the Progressive era, controversy over materialism and modernity in the 1920s and 1930s, and cultural critics since World War II. There are many useful sketches of religious, political, and literary figures who voiced critical, often moralistic, assessments of the increasing complexity of American life, but the book has little to say about the simple life in actual experience or as viewed by public opinion. Shi does look at the writings of John Burroughs, Scott Nearing and Helen Nearing, and a few other practitioners of plain living, but even in these cases he usually concentrates on their public pronouncements rather than on the quality of their experiences. Discussion of public opinion is infrequent and generalized.

It is not entirely clear that the book is about the simple life, although it repeatedly reifies the term. Many of the figures whose writings are quoted seem to have preferred some version of the "middle way" (the Calvinists [p. 15]), "the mid-world" (Ralph Waldo Emerson [p. 139]), the "border life" (Henry David Thoreau [p. 149]), or "innovative nostalgia" (Frank Lloyd Wright [p. 187]). Their goal was moderation, not austerity; William James spoke of the "physical luxury of simplification" in his spacious summer house (p. 172). Shi's interpretive stance is usually ironic. He presents history as a series of oppositions: in the colonial period, between Christian spirituality and the prerogatives of rank; in the early national period, between republican simplicity and material self-interest; and in later periods, between old-fashioned values and technological progress. In this scheme the pursuit of simplicity has ordinarily been frustrated because its adherents simultaneously accepted contradictory goals. Yet the chief message of the epilogue seems to be that proponents of the simple life should "acknowledge that material progress and urban life can frequently be compatible with spiritual, moral, or intellectual concerns" (p. 279).

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Readers of this book will frequently be reminded of Edmund S. Morgan on the Puritan dilemma, Marvin Meyers on venturous conservatives, Warren Susman on culture and abundance, and other important interpretations of American culture. *The Simple Life* makes its own modest but sensible contributions, but perhaps its greatest service is to restate some tensions and paradoxes that remain central to historical understanding of America's great transformation.

LEWIS PERRY
Vanderbilt University

JOHN DILLENBERGER. *The Visual Arts and Christianity in America: The Colonial Period through the Nineteenth Century.* (Scholars Press Studies in the Humanities Series, number 5.) Chico, Calif.: Scholars. 1984. Pp. 225. Cloth $29.95, paper $19.50.

This book surveys the "currents in which the visual arts and Christianity intersected" in American culture before 1900 (p. 1). The only previous work to address the subject was the *The Hand and the Spirit: Religious Art in America 1700–1900,* (1972), an exhibition catalogue by Jane Dillenberger and Joshua C. Taylor, who chose and analyzed works that revealed what they called "the religious impulse" in Protestant art. John Dillenberger's book is more inclusive and at the same time less interpretive.

The essays in his first section, on Protestant and Catholic approaches to the visual arts, are the most satisfying because each essay is unified culturally. Dillenberger argues that New England attitudes toward art can be traced to the English Lollard tradition, which exalted the Word, and he demonstrates the matter-of-fact use of painting and sculpture by Catholic churches in the Southwest as aids to worship.

The book's second and third sections are composed of a number of short chapters that address different aspects of religious art in nineteenth-century American life. In looking at the viewpoints expressed by clerygmen, religious journals, artists, and architects (each handled in a separate chapter), Dillenberger is most helpful in discussing religious journals and the visual arts. In the material on artists, he leads us to no conclusions; indeed, he states that there was little if any relationship between artists' private religious beliefs and their religious art. But in his analysis of the journals, he demonstrates that sympathetic assessments of the role of the visual arts gradually evolved over the century. In his third section, entitled "Nineteenth-Century Art in Private and Public Life," he looks at religious attitudes expressed toward nature and landscape painting, civil religion as found in sculptures of national leaders and paintings with national themes, works with Old Testament themes, and works depicting Christ.

Although Dillenberger has brought together many aspects of religion and the arts, including a generous number of illustrations and substantial bibliographical references, his failure to sustain a flow of analysis throughout the separate chapters in the book is disappointing. While he claims that he is providing "a picture of the range, scope, and place of (religious) works in the American cultural scene" (p. 123), he seems not to believe that much cultural analysis is possible. In his epilogue he identifies religious art in American life as an epiphenomenon of the cultural context (p. 157).

ELIZABETH JOHNS
University of Maryland

THOMAS G. DYER. *The University of Georgia: A Bicentennial History, 1785–1985.* Athens: University of Georgia Press. 1985. Pp. x, 435. $35.00.

In commemoration of the University of Georgia's bicentennial Thomas G. Dyer has written a clear and concise account of that institution's evolution. Reporting the life of a university is no easy task, but this work is well organized and soundly documented, with careful use of the extensive manuscript sources available. The reader, however, never gets a distinct impression of campus spirit or student life styles. In addition, for the post-1950 era critical sources were not available; thus, the author's opportunity to demonstrate the spirit of the times was restricted. But the reader is introduced to important scenes in legislative chambers and in courts where critical battles took place. Dyer has prepared a comprehensive account of the university's long, tortuous development, from the infant establishment through its uncertain adolescence to the achievement of status as a legitimate university only late in the life of the institution.

From the beginning in 1785 the school, then called Franklin College, benefited from a charter that allowed a liberal drift in matters of religion and military service. Fortunately, at the start two Yale graduates were successful in seeing that state lands were made available to help support "a college or seminary of learning." But the university was plagued by a score of obstinate problems. For most of the past two hundred years the Georgia legislature failed to provide adequate financial backing; during the first hundred years, for example, the state appropriated a total of only $81,400, which included the value of the original grant of land. Later, by adjoining the land-grant agricultural college to the existing university, the financial drought eased somewhat, but other difficulties emerged as a three-way struggle developed when the university,

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its semi-independent college of agriculture, and the Georgia Institute of Technology lobbied separately for prestige and funds.

Unfortunately, for most of its life the institution existed within a shell of restricting social traditions that prevented it from becoming a true university. Until after the Civil War students were white, male, southern-born, and generally from families enjoying above-average financial resources. Enrollment remained officially closed to women until World War I and to blacks until 1961. Somewhat confining as well was the tendency of the early Board of Regents to select its presidents or chancellors from the southern Protestant clergy. (Records of student religious preference indicate that Presbyterianism was the predominant denomination.) State political conflicts were forced on university administrative circles, resulting in interference and personal vendettas from time to time, such as under Governor Eugene Talmadge. At one point strains of anti-intellectualism and political interference brought the Southern Association of Colleges and Schools to strip the white colleges of the university system of their accreditation. For at least a generation football fanatics among influential alumni of the University of Georgia (as of many institutions elsewhere) championed pigskin phrenitis while ignoring library and research shortcomings.

Over the years since World War II increased attention has led to consistent upgrading of the university's teaching staff, research programs, and public service activities. Dyer concludes that important aspects of the university have developed phenomenally since World War II. For example, in 1945 only $168,000 was expended on research whereas by the mid-1980s annual support of nearly $75 million was provided for research programs.

JAMES C. CAREY
Kansas State University

PETER L. TYOR and LELAND V. BELL. *Caring for the Retarded in America: A History*. (Contributions in Medical History, number 15.) Westport, Conn.: Greenwood. 1984. Pp. xvi, 215. $29.95.

This is a narrative history of the treatment of the mentally retarded in the United States from the early nineteenth century, when optimistic educators defined idiocy as "a disease of society" and resolved to eradicate it, to the 1970s, when federal legislation and court decisions established the fundamental right of the retarded to reside in neighborhoods under conditions similar to those of other citizens. This history begins and ends with descriptions of ambitious efforts to integrate the mentally deficient into American society, but in between, in the late nineteenth and early twentieth centuries, asylums were built where the mentally deficient were to be segregated from society because they were regarded as a menace to it.

Peter L. Tyor and Leland V. Bell provide good descriptions of the changing attitudes toward the retarded that parallel the broad changes in strategies for amelioration of their condition. The authors are generally sympathetic toward the teachers, psychiatrists, and asylum superintendents who built institutions that "represented environments of concern, not convenience" (p. 153). Grounded in primary sources, well written and chronologically comprehensive, this monograph supplements the large literature on the treatment of the mentally ill and provides a reliable reference for those who are interested in developing an analytical and comparative history of the treatment of the dependent in the United States.

Tyor and Bell are at their best in describing changing attitudes and therapeutics. One hopes that this sound monograph will set the stage for other studies that devote more attention to certain questions. Why, for example, have attitudes toward the mentally deficient changed? Who were the retarded? What were the pressure groups that shaped policy regarding the retarded, and what were the specific means through which they asserted influence? Have public expenditures for the retarded increased in the twentieth century relative to general wealth or to other social welfare efforts? Finally, this reviewer could not find any reference to Willowbrook, the New York institution that has become a symbol of custodial ineptitude and the subject of the recently published *Willowbrook Wars* by David Rothman and Sheila Rothman.

JAMES REED
Rutgers University

DAVID BEERS QUINN. *Set Fair for Roanoke: Voyages and Colonies, 1584–1606*. Chapel Hill: University of North Carolina Press, for America's Four Hundredth Anniversary Committee. 1985. Pp. xxiv, 467. Cloth $19.95, paper $9.95.

American historians who begin their books and courses in 1607 or 1620 are deprived of two great pleasures: the history of North America in the sixteenth century and the works of David Beers Quinn. The two are nearly synonymous. Since 1932 Quinn has written and edited so many first-rate books (twenty-three at last count) and articles (over one hundred ten) that any syllabus on America's first century and a half must consist largely of Quinn. His Hakluyt Society editions and five-volume *New American World* (1979) are models of scholarly craftsmanship; his collected essays, *England and the Discovery of America, 1481–1620* (1974),

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should be required reading for every graduate student in American and European history.

To that awesome bibliography Quinn has added a distinguished title in honor of North Carolina's 400th anniversary. Based on a true mastery of the documents in his *Roanoke Voyages* (1955) and *The American Drawings of John White* (1964), *Set Fair for Roanoke* is more than a timely enlargement of his still-valuable *Raleigh and the British Empire* (1947). It is a full, judicious, definitive statement on Ralegh's star-crossed voyages to America, their international context, and the fate of the "lost colonists" who left Roanoke Island to reside with the neighboring Indians on Croatoan Island and the south shore of Chesapeake Bay.

The book is divided into four parts. The first, consisting of eleven chapters, describes the planning and planting of the first (male) colony under Colonel Ralph Lane. The three chapters of part 2 analyze the rich descriptions and drawings of Thomas Harriot, the expedition's Indian expert, and John White, its draughtsman, and the surprisingly amicable relations of the colonists with the natives. The third part narrates in four chapters the second ill-equipped settlement of men, women, and children, Governor White's immediate return to England to secure supplies and additional colonists, and his inability to return before 1590 because of the Spanish Armada and the English penchant for privateering in the Caribbean. And in what Quinn calls "a conscientious interim report" the final three chapters (part 4) bring the search for the lost colonists up-to-date by describing the latest contributions of ethnohistorians and historical archaeologists (p. xvi). The thirty-two illustrations are integral to the text.

Quinn calls his book "a plain and relatively full story" of the English voyages to North America between 1584 and 1590. If by plain we mean well-crafted, scrupulously fair, and pellucidly clear prose, then his book is plain; there can be no doubt about the fullness of its coverage. Quinn has thought deeply about every available scrap of evidence, several of which he has discovered since 1955. Even more important, he has masterfully placed all the evidence in its proper context and filled numerous lacunae with highly educated guesses that the reader instinctively respects. Although the Roanoke voyages were English, he deftly interweaves their story into the histories of Spain, France, Ireland, the West Indies, and even Newfoundland. His shrewd knowledge of human nature makes quick and plausible work of undocumented motivations and feelings; his sensitivity to the nuances of native culture and to how the coastal Algonquians must have responded to the English invasion is one of the strongest features of the book. In this regard it is equal (if not superior) to Karen Kupperman's much briefer but more expensive *Roanoke: The Abandoned Colony* (1984).

Set Fair for Roanoke may not be the last word on the lost colony, but it is indisputably the best.

JAMES AXTELL
College of William and Mary

CHRISTINE LEIGH HEYRMAN. *Commerce and Culture: The Maritime Communities of Colonial Massachusetts, 1690–1750*. New York: W. W. Norton. 1984. Pp. 431. $25.00.

Each year since the late 1960s one or two New England town studies by professional historians have been published; their collective impact has exponentially increased our knowledge of the day-to-day life of early America. Reviewers of the first wave of these books frequently questioned the typicality of the town under study. Now that many types of communities have received scholarly attention, reviewers often complain about repetitive and formulaic writing. In truth, as all local historians realize, no "typical" New England town exists or ever existed. The only way to make sense out of the varied local experience is to amass a large corpus of community studies that will enable students to identify patterns and trends and to separate the idiosyncratic from the general. This is the process by which historical knowledge advances, and this is what New England historians have been doing. And, in doing so, they have been far less formulaic than past members of some scholarly generations. Compared to the work of the Dunning school of Reconstruction or to the Progressive studies of the revolution, New England town studies sparkle with originality in research design. Moreover, they have developed a wonderful capacity to learn from their immediate predecessors; rather than replicate the earliest studies, the present ones engage them in debates over both substance and methodology.

Christine Leigh Heyrman's analysis of Gloucester and Marblehead, Massachusetts, from their founding to the mid-eighteenth century, exemplifies the present vitality of the best of this work. Both of these northern Massachusetts coastal towns were founded under disorderly circumstances and existed for a generation or two amid near chaos. Neither were populated by quiet Puritans clustered around a meetinghouse. Less-than-devout Congregationalists, arrogant Anglicans, and contentious Quakers moved in and out of town, brawled and fornicated with regularity, and made their living as much from the sea as from the land. Gloucester and Marblehead may have been less than one hundred miles from Sumner Chilton Powell's and Kenneth Lockridge's Puritan villages of Sudbury and Dedham,

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but they were light years distant in terms of their social milieu.

As did most of New England's towns, Gloucester and Marblehead grew and prospered in the early eighteenth century as the northern colonies became intimately involved in the Atlantic trading world. Intellectual and social historians alike have usually argued that economic expansion occasioned a decline in Puritan orthodoxy. Worldly pursuits, rising inequalities of wealth, the end of isolation, the differentiation of occupations, and a hardening class structure are usually regarded as both cause and effect of the erosion of the static communalism needed to sustain the Puritan ethos. Heyrman, however, turns this argument on its head: Gloucester and Marblehead, she argues, became more orderly, more tranquil, and more traditional as they developed from their rough-and-tumble beginnings into settled and prosperous communities. Townspeople moved less often, they developed a sense of local pride and loyalty, and elites emerged that were committed to traditional values of order, family, and church. In short, although she does not explicitly use the terms, Heyrman argues that Gloucester and Marblehead underwent a transition that, with only slight overstatement, could be called "from Yankee to Puritan."

Heyrman does not prove her case beyond a reasonable doubt. Her interpretation is marred by too many conditional words—"probably," "suggests," "likely"—which usually indicate that she is choosing the most convenient of several possibilities. She relies too heavily on individual examples and not heavily enough on hard data. Some comparisons she makes with Chesapeake society are a little strained. But no one reading her book will doubt that Gloucester and Marblehead developed along different lines than most of the towns studied previously. And, as Heyrman argues, other small seaports such as these probably did exist. Her book thus is a valuable addition to local studies for two reasons: first, it adds its measure to the body of information we now have and thus will be useful to others trying to create a large picture from the many town studies; second, it offers a new and provocative interpretation of what economic development may have meant to parts of colonial New England.

BRUCE C. DANIELS
University of Winnipeg

SEAN WILENTZ. *Chants Democratic: New York City and the Rise of the American Working Class, 1788–1850.* New York: Oxford University Press. 1984. Pp. xii, 446. $35.00.

Chants Democratic, Sean Wilentz tells us, is an attempt to give new meaning to a familiar "history of class relations and labor movements in early-nineteenth-century New York [City]" (p. 18). He accomplishes more than that. Dimly remembered men and affairs—John Commerford and Cornelius Blatchly, the Astor Place riot and the New York Industrial Congress—come richly to life through his narrative. Subjects that historians generally address in ones and twos—industrial production, ideological currents, labor militancy, party politics, street culture, nativist sentiments, and more—Wilentz invites us to explore in fascinating sets of connections. Neither depth nor breadth burdens the book because Wilentz writes with verve and skill. It is a pleasure to read his account of an emerging working class that between the late 1820s and 1850 reshaped America's republican ideology into an attack against laissez-faire capitalism, particularly its wage system.

How persuasively Wilentz argues this case is another matter. Social relations no doubt changed fundamentally during the early nineteenth century. But did a class specifically based in the New York City trades materialize by mid-century? The evidence is exceedingly thin. The labor movements at the heart of Wilentz's story turn out merely to be labor moments, several months of dramatic action in 1836 and a few more in 1850. Whether the radical voices that were raised before these outbursts had special relation to a body of wageearners and whether more than one or two strays saw these widely separated moments as parts of a common process remain open questions. Nothing in Wilentz's evidence contradicts Herbert Gutman's outline of repeated starts toward a workers' tradition repeatedly lost. From time to time Wilentz acknowledges these qualities of elusiveness and fragmentation, but he manages to do so in ways that actually assert the opposite; for example, "Strikes, although fast becoming a fact of workshop life, remained something of a novelty" (p. 60). In the end, Wilentz's case requires an a priori commitment to a distinguishable working class, despite his own warnings against this kind of argumentation.

Wilentz's economic setting poses additional problems. In some areas, such as his intriguing linkages between weakening skills and rising militancy among workers, he takes considerable pains. Other matters, such as the impression of an apparently unrelieved decline in workers' real income between the 1810s and 1850, cry out for greater precision. Did this blight hit all categories of workers indiscriminately? How did cities maintain an appearance of opportunity and continue to draw floods of newcomers? Are we to picture an even more devastating decline affecting the countryside? The history of class and a good deal else in early nineteenth-century America would seem to hinge on the answers.

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These puzzles lead to a third area of difficulty. What, in fact, is the relation between New York City and the rest of the nation? Although Wilentz does not answer directly, he suggests that the city moved at the forefront of developments for the entire society. New York City was "the metropolis of America" (p. 6), he declares, comparable in importance to the United States as London to Britain or Paris to France. It requires a lofty level of abstraction with eyes set toward the future to hazard such a claim about the sprawling, decentralized America of the early nineteenth century. The merits of Wilentz's study lie closer to earth, where he unfolds an exciting account of social transformation in a unique, explosively growing city. Here is a challenging attempt to mate two of the most popular fashions in the writing of American history: the making of a working-class culture à la E. P. Thompson and the role of republican ideology à la J. G. A. Pocock.

ROBERT WIEBE
Northwestern University

BARBARA M. TUCKER. *Samuel Slater and the Origins of the American Textile Industry, 1790–1860.* Ithaca: Cornell University Press. 1984. Pp. 268. $29.95.

In the 1790s Samuel Slater established the first commercially successful water-powered cotton mills in the United States, and the business records pertaining have survived in the Almy and Brown and Slater collections at Harvard. Barbara M. Tucker has reexamined these and other documents, as well as the extensive secondary literature on the early American and British textile industry, to provide a clearer understanding of the origins of Slater's business policies and their effect on workers and working-class families.

Tucker at the outset refuses to follow a simplistic class-conflict scheme of analysis, arguing instead that "cultural" factors were as important as class in determining the relations between capital and labor. The cultural factors she refers to are sex roles, family structure, rural village life style, and religion, which in New England combined to produce tightly knit rural Protestant communities composed of patriarchally organized working-class households. Unable to recruit the necessary number of women and children by the traditional British methods of individual contract and workhouse apprenticeships, Slater turned to another British mode that fitted the American situation better: the family contract. By making an agreement with the male head-of-household, who provided children and young women to work in the mill in return for his own employment outside the mill, Slater temporarily buttressed the position of the paterfamilias threatened by the earning power of his former dependents. The householder collected the family's pay and was even given the customary right of entering the mill to discipline his children.

This system eventually broke down as female-operated power looms became common and factory discipline tightened; young women were paid their wages separately and increasingly lived in boarding houses, Lowell-style; eventually, many New England families sought work elsewhere, their places being taken by Catholic Irish and other European immigrants. But for a few decades Slater's system served to support, not to change, the traditional New England patriarchal family.

The work's one weakness lies in not dealing with an important aspect of cotton-manufacturing technology: the distinction in the labor requirements of the water frame and the mule. At the outset, Slater seems to have re-created the Arkwright system, which was based on the water frame, a continuous spinning device that could be operated by relatively unskilled women and youths; the mule, which became the predominant spinning machine in Britain only after Slater's emigration, was needed for spinning the finer counts of yarn and required a strong, highly skilled male spinner (before the advent of the self-acting mule). Although Tucker alludes in passing to mule spinners, the term "mule" does not occur in the index, and no attention is paid to the contradiction between the early household contracts that kept men out of the mills, supposedly at their own desire, and the later presence of mule-spinners, who in some Pennsylvania mills, at least, were hired in family contracts and were probably so employed by the Slaters. It would be interesting to know the numbers and periods of use of spinning mules in Slater's factories; it has been said that the extensive use of the mule in Rhode Island factories, and in those farther south, differentiated the Rhode Island system from that of Waltham and Lowell. In this sense Slater's first mills were not the prototypes of all that followed, perhaps even in his own business.

But mules and water frames aside, Tucker's work contributes importantly to the growing understanding of the complexity of social relations in the early Industrial Revolution. Sex roles, family structure, religion, and the high valuation of the rural life were as important, in the eyes of many, as social class in determining the politics of industrialization.

ANTHONY F. C. WALLACE
University of Pennsylvania

DAVID A. HOUNSHELL. *From the American System to Mass Production, 1800–1932: The Development of Manufacturing Technology in the United States.* (Studies in Industry and Society, number 4.) Baltimore: Johns Hopkins University Press. 1984. Pp. xxi, 411. $37.50.

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In 1854 the British sent a committee to investigate manufacturing methods in America's federal armories. So impressed was the committee with the extent of mechanization and interchangeability that its report hailed the appearance of a distinctly American "system of manufacturing." Today, however, the system of mass production is cited as one of the major causes of America's productivity problem and relative decline internationally. In this book David A. Hounshell systematically studies the evolution of some of America's most important manufacturing technologies. Locating their origins in the federal armories, Hounshell traces them from the clock, sewing-machine, and woodworking industries to reapers, bicycles, and autos. He uncovers the strategic role of a close-knit group of mechanics who, moving in and out of these industries, exchanged and disseminated ideas and, in the process, became almost as interchangeable as the parts they interchanged. By the late nineteenth century they had bequeathed to American engineers and manufacturers an array of manufacturing methods.

According to Hounshell, these changes in manufacturing stretched the American system to its technological limits. True production for the masses came just before World War I, when the Ford Motor Company installed a moving assembly line to break the bottleneck in production that had been formed by mass production of interchangeable automobile parts. Hounshell credits Henry Ford with launching the mass production revolution, but he remains critical of Ford's overall contribution. Whereas mass production under Ford's tutelage became increasingly inflexible, flexibility was built into mass production at General Motors in the 1920s when it pioneered its policy of annual model changes. In a concluding chapter Hounshell describes how the triumph of mass production over a wider range of industries diluted its initial heroic aura of innovation and efficiency.

Hounshell does not presume to instruct today's industrial policy makers with this book. Rather, the policy lessons emerge from between the lines of his detailed, exhaustive history: do not assume, simplify, or generalize about the evolving behavior of different American manufacturers. They did not all share the same goals nor face the same problems, and the allure of new manufacturing methods did not automatically make old methods disappear. Whereas early armsmakers developed interchangeability as a military expedient to answer the government's demand, clockmakers seized on it as a way to produce a number of cheap clocks and satisfy a growing demand. Singer, on the other hand, stubbornly clung to outdated techniques of European manufacture until the 1880s, when domestic competition and the growing foreign demand for standardization forced it to mechanize its operations.

The book contains over one hundred sixty magnificent illustrations, which Hounshell uses as documentary evidence, along with company archives, letters, personal diaries, and trade and business journals, to show the reader how methods and technologies actually operated and evolved. The result is that his book is like a "modern moving picture machine," a joy to read and see (p. 245).

As with any good book, Hounshell's interpretation is bound to be controversial. His characterization of the productivity dilemma as "the problem of choosing between frequent product changes and lower productivity or no change and higher productivity" (p. 13) is not likely to sit well with measurement-minded economists or historians, and Hounshell makes no attempt to measure anything. Furthermore, he occasionally becomes too involved in academic nit-picking over chicken-and-egg-type questions, which, though he resolves them efficiently, do interrupt the flow of his history.

But such criticisms are minor given the significance and the quality of Hounshell's contributions. He not only charts new directions for the study of technology but also comes close to bridging the wide gap between business history and the history of technology. Moreover, his book addresses problems of importance in today's economy without sacrificing quality history. It informs without teaching and converts without preaching. Hounshell's achievement is nothing less than a feat of historical conjury: his book brings machines to life.

MARY A. YEAGER
University of California, Los Angeles

LYNN Y. WEINER. *From Working Girl to Working Mother: The Female Labor Force in the United States, 1820–1980*. Chapel Hill: University of North Carolina Press. 1985. Pp. xii, 187. $17.95.

This book analyzes the changes in women's patterns of participation in the American labor force since the appearance of the factory system as the transformation of the typical wage-earning woman "from working girl to working mother." Lynn Y. Weiner's focus is the public debate and policy responses that these changes stimulated.

The book has two parts: "The Era of the Working Girl, 1820–1920" and "The Era of the Working Mother, 1920–80." Each part is divided into three chapters that discuss the forces behind and changes in participation of women in the labor force, public reaction to these changes, and the public policy responses. Weiner argues that public policy in the first period was focused on protecting white, middle-class working girls for future motherhood and took the form of protective labor legislation,

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recreation clubs, and supervised boarding houses. In the second period the concern was for the children of white, middle-class, wage-earning mothers and centered on the issue of day care.

Thus, although she breaks down statistical data and descriptions of occupations by race and nativity, Weiner argues that these native, white, middle-class girls and mothers were the focus of public controversy and, hence, of policy making. "Immigrant and black women have always worked. But when [single] native-born white women increased their propensity to work . . . the question of woman's proper place gathered strength in the public debate" (p. 13). This implicit claim for a white, middle-class hegemony over public opinion and policy making, and a resultant lack of concern about the poor, immigrant, or black, is thought-provoking but never clearly argued, for Weiner claims to be discussing public opinion, not white, middle-class public opinion. Also, this thesis is not upheld by Weiner's own material: in the discussion of policy toward working mothers, she is forced to spend almost half the chapter on the large number of pre-1920 policies toward poor, black, and immigrant working mothers.

Perhaps these lapses can be explained by Weiner's apparent choice to focus on social practices that generated controversy. In the first period, "because white native-born women who worked most clearly violated norms of sex-role behavior, they captured public attention" (p. 36), whereas in the twentieth century "the working mother replaced the working girl as a symbol of female role transgression and social change" (p. 98). But Weiner overplays the extent to which these women deviated from traditional femininity. As Weiner herself points out, a high percentage of the single, purportedly "self-supporting women" were actually giving at least part of their wages to family members, that is, they were working within the feminine role, as daughters or sisters (p. 26). This was also true of the majority of the wage-earning wives, who worked to help with the family finances. These women did not break out of the feminine role but extended it in an increasingly contradictory way. Weiner's own analysis of "public opinion" shows that the concern was less for the sex-role transgressions of these women than for the health and safety of their present or future children. Surprisingly, Weiner barely mentions the sex-role "transgression" of educated women demanding entry into masculine jobs and the debates and policies surrounding this major change.

These weaknesses in Weiner's theoretical framework do not prevent her from presenting a rich, detailed, complex, and valuable picture of this history. Weiner makes excellent use of a broad range of primary sources to bring these wage-earning women of the past alive. And her discussion of the policy debates surrounding women's entrance into the labor force will enlighten contemporary policy debates on the issues of protective legislation, day care, and the poverty of female-headed households.

JULIE MATTHAEI
Wellesley College

ANDREAS REICHSTEIN. *Der texanische Unabhängigkeitskrieg, 1835–1836*. Berlin: Dietrich Reimer. 1984. Pp. viii, 354. DM 48.

In order to flourish, emergent sociopolitical formations need an interpretive belief system that reveals their virtuous and providential origins for future generations. Historical interpretations of the making of a Texas independent from Mexico in 1836 and its annexation by the United States nine years later offer an illuminating example of that myth-making process in which scholars like E. C. Barker, W. C. Binkley, and T. R. Fehrenbach have served as influential formulators. They labeled the events a "revolution," an echo of 1776; that is, a liberation struggle against a corrupt and racially inferior regime. Andreas Reichstein challenges these interpretations. He attempts to discover the real motivations of the main actors and probes the collusion of Freemasons, the machinations of profit-hungry land speculators, and the designs of ruthless expansionists. To achieve his countersynthesis, the author combed Mexican and Anglo-American archival collections, newspapers, printed source editions, and a host of books and articles; he also subjected the unearthed materials to critical comparison to clarify the often-contradictory evidence.

The study opens with a brief historiographical essay and then offers a sketch of events up to 1819 and a chapter on the growing Anglo-Americanization of the Texas territory between 1819 and 1832. The fourth chapter details events that led to the separation from Mexico and the incorporation into the United States. The concluding chapter reviews various interpretations and offers cogently reasoned counterperspectives. Reichstein finds Polk's claim of an aggressive Mexico false and probably fabricated by design. He also refutes the abolitionist assertion of a successful coup of the southern slavocracy. He further rejects the "culture clash" hypothesis proposed by Barker and sharpened by Fehrenbach, who holds that "the underlying reason for revolt was ethnic hatred on both sides" (*Lone Star* [1968], p. 168). Reichstein criticizes most strongly the view that the ideology of "manifest destiny" inspired the events; instead, he views manifest destiny as a "fig leaf" (p. 206) designed ex post facto to conceal a morally bankrupt policy of naked power.

The study has many merits. The descriptive chapters are filled with facts and judiciously crafted. The

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interpretation, though in conflict with patriotic lore, is based on evidence and sound critical judgment. Yet the author underplays the fact that the Anglo-American conquest was a second round of displacement: Anglo-American aggression merely replaced Spanish-Mexican aggression against the region's Indians and both were forms of naked *Machtkampf* (p. 197) in the context of Caucasian expansion. But Reichstein undoubtedly offers a far more convincing view of the events than the traditional approaches whose justificatory thrust he so ably exposes.

LEO SCHELBERT
University of Illinois, Chicago

WILLIAM E. UNRAU and H. CRAIG MINER. *Tribal Dispossession and the Ottawa Indian University Fraud.* Norman: University of Oklahoma Press. 1985. Pp. xii, 212. $17.95.

William E. Unrau and H. Craig Miner have written a short monograph that surveys the tragic history of three bands of Ottawa Indians who lived in northeastern Ohio. The book provides a summary of Ottawa-white relations from 1600 to 1822, a brief discussion of events leading to the Ottawa removal to Kansas and Oklahoma, and the belated justice that these Indians eventually received under the Indian Claims Commission Act of 1946. Most of this book is a detailed account of a land fraud involving the Baptist speculators who established Ottawa University in eastern Kansas.

After their removal to Kansas in the 1830s, the Ottawa were victimized by Baptist religious opportunists, who included Jotham Meeker, the first resident missionary at the Ottawa Baptist Mission; John Tecumseh (Tauy) Jones, a mixed-blood trader; Clinton Carter Hutchinson, a land speculator and administrator of the Ottawa Indian Agency; and Reverends Isaac S. Kalloch and Robert Atkinson, who were affiliated with the Baptist Home Mission Society. According to the authors, these men were interested in material profit more than moral issues.

In 1862 these wilderness entrepreneurs betrayed the Ottawa by persuading them to sign a treaty that provided for land allotment and the creation of an Indian school, which would be built and endowed with funds from the sale of 20,000 acres of tribal land. Hutchinson, Kalloch, and Jones used this treaty and their authority as members of the board of trustees at Ottawa University to engage in fraud, land speculation, and theft. Over $31,000 received from the sale of school land disappeared, and unauthorized tribal trust property was sold to encourage white settlement rather than to promote Indian educational opportunity. Two congressional commissions investigated this scandal but were prevented from taking effective action by Reverend Atkinson and people who supported the Baptist Home Mission Society.

Discouraged by these events, the Ottawa signed a second removal treaty in 1868 and relocated to a 12,000–acre reservation in Oklahoma. A few years later, they were included under the provisions of the Dawes General Allotment Act and eventually sold most of their homesteads. In 1956 the Ottawa approved a termination bill because they concluded that this decision would expedite their case before the Indian Claims Commission. In 1965 the Ottawa were awarded $406,000 for violations of their treaty of 1862 and the improprieties associated with the Ottawa University fraud.

This study has shortcomings. At times, the narrative becomes too detailed and the book does not have a concluding chapter that analyzes the broader significance of the Ottawa University episode. The authors often seem more interested in passing moral judgments than in relating to the general context of what was happening in American society.

Nevertheless, Unrau and Miner have contributed to our understanding of Ottawa history and Indian removal in the North. They convincingly demonstrate that many missionaries who advocated assimilation were not guided by the highest motives. The authors' discussion of how Ottawa treaties were manipulated to advance white economic interests helps explain why Congress ended treaty making in 1871.

KENNETH R. PHILP
University of Texas, Arlington

NAOMI W. COHEN. *Encounter with Emancipation: The German Jews in the United States, 1830–1914.* Philadelphia: Jewish Publication Society of America. 1984. Pp. xiv, 407. $25.95.

If pluralism has become a hallmark of America, so it is also a significant theme in American Jewish history. In her comprehensive work Naomi W. Cohen offers impressive evidence to claim that "it was the German Jews—not the Sephardic pioneers nor the numerous Russian Jews—who laid the foundations of the modern American Jewish community" (p. xi). Although historians may differ on conclusions, the tensions between ethnic and religious identity described in this book form a fascinating pattern with the goals of acceptance and integration.

For German Jews, Americanization seemingly began even before they touched these shores. Many embraced the ideology of natural rights, individualism, laissez faire, and emergent secularism. For them, unlike the more traditional Sephardim and

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East Europeans, the constraints of religious community had been weakened by the promise of democratic emancipation. In Germany that promise was thwarted by failed revolutions and new disabilities. In the New World liberty could be fulfilled. With their economic and geographic mobility amid unparalleled expansion, German Jews developed new communities and institutions in the emerging western heartland of America.

Cohen's thematic approach goes beyond ethnic and religious issues, to include lucid analyses of contributions to the economic growth of the United States. Peddling, store-keeping, and pioneering in ready-made clothing offered upward mobility for many, and a few even had exceptional success in banking and finance. Familiar themes are developed here with scholarly perspective, balanced anecdotal detail, and full discussion of hardships and humiliations along with progress. Controversy over Sunday blue laws, for example, continued into the twentieth century. By then, the German Jews who had generally adapted to such public constraints, urged their East European brethren to do likewise.

But there was always the minority's self-conscious concern with image, when confronted with charges of social separatism (while also "obtrusiveness") or with double standards in the marketplace. Rabbi Isaac Leeser of Philadelphia cautioned Jewish employers in 1855 that more was expected of them in labor relations, so they should pay the highest wages, "if possible . . . a little more than non-Israelites" (p. 113). Similar sensitivities would play a role in the great cloakmakers' strike of 1910, which pitted Jewish employers of the established community against the often "radical" Jewish workers of the new immigration.

Self-criticism and defensiveness concerning "excessive" commercial accomplishment brought pleas for a virtuous agrarianism. Cohen weaves this theme, too, into historic context, describing the varied back-to-the-soil activities of organizations and individuals (such as Adolf Brandeis, father of the first Jewish Supreme Court justice). Nowhere did the lofty prose of German Jewish rhetoric glitter as much as in extolling the virtues of farming. Yet, as the author notes cogently, this prescription for East European Jews crowding the urban ghettos early in the twentieth century was typically humanitarian while also defensive.

Ultimately, the German Jewish community's genius lay in its institutional structure, its shaping of Reform Judaism, philanthropy here and abroad, and a fervent Americanization. In the words of a Charleston rabbi dedicating a house of worship in 1841: "This synagogue is our *temple*, this city our *Jerusalem*, this happy land our Palestine" (p. 170).

JOSEPH BRANDES
William Paterson College

JUDITH E. ENDELMAN. *The Jewish Community of Indianapolis, 1849 to the Present.* (Modern Jewish Experience.) Bloomington: Indiana University Press. 1984. Pp. ix, 303. $17.50.

The distinctiveness of Indianapolis Jewry emerges in clear perspective from Judith E. Endelman's study. An ethnic community of about eleven thousand, approximately 1 percent of the total population, Indianapolis Jewry constituted the largest single foreign-born group in the city in the early twentieth century. Although Indiana had an exceptionally high percentage of white, native-born Protestants largely engaged in rural pursuits, Hoosier Jews successfully maintained their distinctive cultural and religious traditions through a process of accommodation to their environment. Despite the existence of Silver Shirt headquarters in Noblesville, Indiana, and the resurgence of anti-Semitism, the author notes a lesser degree of virulent anti-Jewish prejudice in Indianapolis than in other midwestern cities.

Indianapolis Jewry was always in the forefront of inter- and intracommunal cooperation. Although the community developed its own Jewish Welfare Fund, it also derived in the early years a significant proportion of its funding from the Community Chest. The program of the Jewish Educational Association represented the merged efforts of different religious groups, a pattern found in few Jewish communities.

Demographic analysis of changing communal patterns are a feature of this study. A product of the "new history," the book is replete with statistics concerning institutions and attitudes of this Hoosier community. Oral interviews lend a flavor of authenticity to the author's conclusions.

From a scholarly perspective, however, there are lacunae that deserve elaboration. In too many instances the author derives conclusions from secondary sources, which she quotes to excess. Some of her generalizations are questionable. For example, the assertion that "restrictionists, whose ranks were growing in proportion to the increasing numbers of immigrants" (p. 95) can be contradicted by statistics showing that in the 1890s, when immigration decreased, restrictionism was at a peak. Moreover, the argument that smaller Jewish communities escaped the anti-Semitism found in the major centers of Jewish population cannot be explained solely by the former's high degree of assimilation. The characteristics of the particular American environment that encouraged this integration should also be explored. Some middle-sized Jewish communities such as Atlanta also suffered outbreaks of anti-Semitism.

Despite a proliferation of communal statistics, the author pays little attention to Julian Freeman, an

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outstanding communal leader who became president of the national Council of Jewish Federations and Welfare Funds. More thorough research into Herbert Ezekiel and Gaston Lichtenstein's *History of the Jews of Richmond* (1917), *The Occident* (1867), and *Beginnings on Market Street* (1976) and the reviewer's *Richmond Jewry* (1979) would have supplied for the author full documentation about the careers of Julius Wechsler and S. Berman, early religious functionaries of the Indianapolis Hebrew Congregation. On two occasions, the author uses the term "rahmanot" for piety rather than "rahmanut," perhaps a minor matter but still annoying.

Nevertheless, the author and the editors of the Modern Jewish Experience series are to be commended for this recent addition to the growing library of Jewish communal studies. As more individual communities are investigated, definitive histories of the American Jewish community cannot be too far behind.

MYRON BERMAN
Richmond, Virginia

LEONARD J. ARRINGTON. *Brigham Young: American Moses*. New York: Alfred A. Knopf. 1985. Pp. xvii, 522. $24.95.

"The most tragic event in Mormon history," writes Leonard A. Arrington, was the famed Mountain Meadows massacre of 1857 (p. 257). The most tragic event *about* Mormon history Arrington alludes to at the end of the introduction to "Notes and References": a few of the documents available to him "have since been closed to researchers, and it will not be possible for readers of this book to check out every source I have used" (p. 433). Elsewhere he refers to papers of John Taylor, Brigham Young's successor, which "are known to have existed in the Church Archives in the 1920s but . . . are not there now" (p. 503). Mormons do not lose such things; the official leaders have, however, placed increasing constraints on Saints and "Gentile" historians.

This fine biography forces one to say "more's the pity." Embarassments exist in the records of all churches, and any in the Mormon archives will be sufficiently known from other sources. Suspicion only grows when records are closed. Arrington, however, was a former director of the church's history division and had access to mountains of documents; his register on Young is seventy single-spaced pages long. This made possible perspectives that enable us to sweep the shelf bare of all the bad biographies of this major American religious leader. Mormons, more than any other American religious group, live by story, by history; few bodies have a historical industry or talent pool to match theirs. Biographies such as this show the wisdom of keeping records open.

Biographies such as this . . . In what does its suchness consist? One could speak of it as old-fashioned, brisk, eloquent narrative prose. Sometimes the reader may wish that Arrington had slowed his pace and played ever so cautiously with psychologically informed history. Although he quotes thousands of lines from diaries and letters, something about Young's inner life eludes us. The problem may be Young's as much as Arrington's, however. Young was a busy man, relatively unlettered, intuitive but not spiritually profound. The few paragraphs devoted to what in other religious leaders would be called their theology show how little Mormons needed theology or Young displayed it.

Historians who deal with dull and timid sorts or novelists whose characters have to seem plausible may look in envy on Arrington for his subject. His Young is neither "the Lecher, presiding over the comic opera of his many-wived household; or Brigham the Tyrant, stark" (p. xiii). With astonishing success Arrington creates a "plausibility structure" that makes Young's often-incredible decisions and achievements look credible. The non-Mormon reader, more than one might anticipate, suspends disbelief in the system to follow the figure. "We can try to be fair, to seek balance in our understanding, and to exert . . . empathy" (p. xvii). Arrington's credo may find a match in the readers who generally trust him with a trust that Arrington has previously earned and here recertifies.

Very few flaws appear in the style, which is free from clichés and unburdened by flourishes. One could complain that not enough context from the non-Mormon world appears here. Comment on it would have lengthened a necessarily long book, but it would have kept that non-Mormon world more plausible. Overall, this is an impressive achievement. It shows that the Mormons are less a church than a people—Arrington notes that they have their own category in *The Harvard Encyclopedia of American Ethnic Groups*—and that no one, not even founder Joseph Smith, had as much to do with forming this peoplehood as did their paradoxical first first president. In Arrington, Young has found the biographer he deserves, and he will be available now for a reading public to make of him what it will.

MARTIN E. MARTY
University of Chicago

ROBERT D. HABICH. *Transcendentalism and the* Western Messenger*: A History of the Magazine and Its Contributors, 1835–1841*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1985. Pp. 208. $27.50.

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The proliferation of magazine literature in antebellum America was a trend of substantial historical importance. Magazine reading is one index of growing popular literacy, and magazine writing is one way to probe social standards in a culture where old guidelines often no longer apply. Religious periodicals helped draw denominational boundaries, labor journals enhanced working-class consciousness, and the wide circulation of many publications promoted national unity. Robert D. Habich adds to our understanding of the social implications of magazine journalism with his study of the *Western Messenger*, a monthly published in Cincinnati and Louisville by friends of the New England Transcendentalists. Borrowing questions and methods from intellectual and social history as well as from literary and religious studies, the author reaches his conclusions through interdisciplinary analysis.

Habich comments on issues of interest to historians in various fields. Intellectual historians may find value in his argument on the relation of means and ends: as a joint venture of young Unitarian ministers, the *Western Messenger* had an identity of its own, and its fortunes influenced the ideas and lives of the contributors much more than their experience shaped its content. Social historians concerned with westward migration may learn from the frustration of these easterners, who went west in hopes of finding personal liberty and helping to create a new society but found life in the Ohio Valley fixed and resistant to change. Regionalism also affected the *Western Messenger*'s success: New England readers were cool to its promotion of a distinctly western literature, and western supporters grew tired of reports of contention between Unitarians and Transcendentalists around Boston. For historians of Transcendentalism in particular, Habich reassesses the movement's intellectual boundaries. Scholars have generally viewed the *Western Messenger* as the first Transcendentalist periodical, but Habich contends that the westerners were less like their New England associates in philosophy than, more loosely, in their commitment to intellectual freedom.

The accomplishments of this book are not insignificant, yet it falls short of satisfying answers to the questions raised. The literary, social, and religious contexts of the magazine's publication are not discussed in sufficient depth to let the reader fully grasp the journal's historical role. Too often Habich rests content with a narrative of the magazine's business affairs, a simple report of its contents, and biographical notes on its authors. In addition, some of Habich's questions deflect attention from issues of greater importance. The intellectual differences among the Transcendentalists are well known, as he says, but why some thinkers, such as this group, were more philosophically cautious is a study in comparative biography that needs to be undertaken. Along the same lines, the features of antebellum society that facilitated the formation of an intellectual community that could tolerate internal disagreements bear more thorough analysis than Habich gives.

Although this book does not explore such interpretive possibilities, it remains a useful account of one episode in American intellectual history. Habich reminds us that literary activity has social importance and invites further work in the field.

ANNE C. ROSE
Carnegie-Mellon University

RALPH LUKER. *A Southern Tradition in Theology and Social Criticism, 1830–1930: The Religious Liberalism and Social Conservatism of James Warley Miles, William Porcher Dubose, and Edgar Gardner Murphy*. (Studies in American Religion, number 11.) New York: Edwin Mellen. 1984. Pp. ix, 463. $39.95.

This fine book explores the liberal theology "of the South's white social elite over the span of a critical century from 1830 to 1930" (p. 1). In the thought of James Warley Miles of Charleston, William Porcher DuBose of Sewanee, and Edgar Gardner Murphy of Montgomery, Ralph Luker finds convincing evidence that southern intellectual and religious life was rich and diverse, though these "broad church" Episcopalians supported southern social biases as fully as did their evangelical neighbors.

Miles, a well-born South Carolinian, was rector of several of his home state's most prestigious churches and held a professorship at the College of Charleston in between sieges of depression. He traveled abroad, including a four-year residence in Constantinople as a missionary. A thoroughgoing transcendentalist, he both read and was read by some of the leading intellectuals in Europe. The connection between Miles and DuBose was slight (Luker's "tradition" becomes clear only with DuBose), but they drew on the same sources of European philosophical idealism and theological modernism. DuBose spent nearly fifty years teaching the descendants of the Old South's finest families amid the genteel poverty of Sewanee. Beginning in the 1890s, he wrote several books that reflected his life's devotion to Aristotle, a theory of the Incarnation that saw God working out his divine purpose in history, and a broad plea for catholicity in the body of Christ. Murphy, the brightest of DuBose's Sewanee students, was less a theologian than the other two, but he crafted a social gospel message based on their liberal theological tradition. In 1901, after serving a number of churches as rector, including the prestigious St. John's Church in Montgomery, Murphy left the ministry to become executive secretary of the Southern Education Board. Based on his liberal

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theological heritage, he sought to improve southern race relations, worked to abolish child labor, and promoted public education.

Luker's account of the lives and thought of these three influential southern Episcopalians is superbly researched and deftly told. Although Miles's and Murphy's lives beg further explanation, Luker is especially skilled as an intellectual historian. His exploration of Miles's and DuBose's European connections highlights the degree to which southern Episcopalians conversed with Anglican theologians rather than with liberal thinkers in America.

Much of Luker's effort goes toward explaining how these cosmopolitan, southern Episcopalians used their theology to support the conservative social values of their section—first in defense of slavery, then the Confederacy, and finally the disfranchisement and segregation of blacks. Miles and DuBose believed in a "polygenetic theory" of racial origins; all three espoused a type of "primal pluralism" arguing that all social groups had distinctive missions in history.

It has become increasingly clear in recent years that nineteenth-century southern religion was more than an ongoing evangelical camp meeting. The liberal Episcopalian tradition was not a large one, but Miles, DuBose, and Murphy ministered to some of the most influential southerners of their times. Their thought tells much about both the intellectual and social diversity of the South and its nethermost unity.

DAVID EDWIN HARRELL, JR.
University of Alabama

HORACE W. RAPER. *William W. Holden: North Carolina's Political Enigma.* (James Sprunt Studies in History and Political Science, number 59.) Chapel Hill: University of North Carolina Press. 1985. Pp. xvi, 376. $29.95.

Until recently, no biographer has ventured to unravel the convoluted political odyssey of North Carolina's William W. Holden. A political journalist initially tied to the Whig party, Holden switched allegiances to edit the Democratic state newspaper and helped to bring that party to power in the 1850s. North Carolina's most ardent supporter of southern rights during the territorial crisis of 1846–51, he became its most outspoken Unionist during the secession crisis.

When the state finally did secede, Holden went along. But, quickly disenchanted with the government's conduct of the war, he became a leading critic of Jefferson Davis. Holden was instrumental in founding the successful Conservative party, only to break with most of its members in 1864. A leader in the state's peace movement, he ran unsuccessfully against the incumbent Conservative governor that year.

Appointed the state's provisional governor in 1865, Holden again failed to win a popular mandate in elections held later that year. In 1866 he cast his lot with the Republicans and supported congressional Reconstruction. He finally won a race for the governorship in 1868 but had little opportunity to savor his victory; he became embroiled in an unsuccessful military effort to suppress the Ku Klux Klan. When the Klan-aided Conservative-Democrats gained control of the state legislature in 1870, they impeached, convicted, and removed Holden from the governorship for violating his oath of office. The Republican party provided Holden with patronage positions, but, after he was removed from office in 1881, he withdrew from the Republican organization and spent the last decade of his life as an independent.

Holden's story offers the biographer a wonderful tool for the study of antebellum political journalism and party organization, the response of the Upper South to the sectional crisis and secession, dissent in the Confederacy, and the rise and fall of southern Republicanism. But not until 1982 was the first biography of Holden published: Bynum Shaw's inadequate revision of Edgar Folk's 1934 doctoral dissertation. Now we have Horace W. Raper's sympathetic but not uncritical biography, a revision of a dissertation completed in 1951.

Raper's volume provides a solid narrative account of Holden's life but is stronger on Reconstruction than on the early years. It also corrects the vicious Dunningite portrait of Holden, an effort rendered somewhat unnecessary by recent Reconstruction historiography. Although this volume brings much of its bibliography up-to-date, it fails to integrate recent findings. Furthermore, Raper ignores some published works and virtually all recent doctoral dissertations, including James Lancaster's superb study of North Carolina scalawags.

Readers will complete Raper's biography knowing much more about Holden's life than ever before, but they will likely understand this complex man no better. The author, unfortunately, substitutes assertion for explanation. For examples, he asserts but does not attempt to prove that Holden was consistent on sectional issues during the 1850s, and he simply attributes Holden's switch to the Republican party to his state and national patriotism. Because Raper never confronts his subject in all of his fascinating complexity, Holden remains an unexplained enigma. Nor does Raper exploit this political life for insights into the events and processes impinging on it. Still, this book merits attention.

MARC W. KRUMAN
Wayne State University

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JÖRG NAGLER. *Fremont contra Lincoln: Die deutsch-amerikanische Opposition in der Republikanischen Partei während des amerikanischen Bürgerkrieges.* (Europäische Hochschulshchriften, 3d series, Geschichte und ihre Hilfswissenschaften, number 246.) Frankfurt a/M.: Peter Lang. 1984. Pp. 411. 74 FR.

In this book Jörg Nagler attempts to delineate the radical German-American influence on the political movement culminating in the nomination of John C. Frémont (the "Pathfinder") on a "radical democratic" ticket in 1864. Strongly attracted by the opportunity afforded by the Civil War to reconstruct society according to the liberal ideas for which they had fought in 1848, certain former German revolutionaries became more and more alienated by the cautious policies of the Lincoln administration. In particular, the revocation of Frémont's Missouri emancipation edict in August 1861 focused German-American interest on the Pathfinder, whose friendship for immigrants was well known. In stressing Frémont's appeal to Germans, Nagler writes that Frémont's charisma provided an opportunity for German-American leaders to attempt to unite their divided compatriots in a political crusade for emancipation and reform. In this meticulously researched account of the growth of German-American opposition to Abraham Lincoln, particularly in Missouri, Nagler seeks to examine the workings of an immigrant pressure group.

Thoroughly at home in the history of radical Republicanism, Nagler has exhaustively mined the private papers of prominent German-Americans and their correspondents. His use of a wide range of German-American newspapers, as well as his knowledge of the sources, is commendable, as is his interesting appendix of pertinent letters, poems, and illustrations. His account of the Frémont movement thus fills a need that has long been neglected.

Nagler's approach, however, creates certain difficulties. Being totally committed, as he is, to the influence of the forty-eighters and other radical refugees, he tends to neglect the large number of German-Americans who remained within the Democratic fold. In addition, he does not show any appreciation of Lincoln's own radical tendencies. The president's skill in balancing radical pressures with conservative countermovements while gradually advancing his own policy was as evident in Missouri as elsewhere, although it seems to have escaped local radical critics. It is also regrettable that Frémont's military and personal shortcomings are not emphasized. And, if German-Americans were angered by General Franz Sigel's loss of command, then Nagler should have mentioned the general's lackluster record at Wilson's Creek and his defeat at New Market.

The extent of the Frémont movement among German-Americans is also problematical. In fact, those German ethnic leaders with the greatest political experience tended to distance themselves from Frémont. Neither Carl Schurz nor Gustave Koerner, for example, ever broke with the administration; Schurz, for one, recognized Lincoln's outstanding abilities earlier than most. If these men, who served as important role models for their countrymen, were not impressed by the Frémont movement, one wonders how widespread was Frémont's popularity among the German-American leadership as a whole. Finally, there is no longer any question about the circumstances that resulted in the Pathfinder's withdrawal from political life and Lincoln's dismissal of Postmaster General Montgomery Blair. As letters in the Samuel F. DuPont Papers make clear, there was no direct bargain, but Zachariah Chandler maneuvered things in such a way as to bring about Blair's retirement immediately after Frémont's declination.

In spite of these caveats, *Frémont contra Lincoln* is a competent work on an important subject. It will add greatly to our understanding of the election of 1864 and its background.

HANS L. TREFOUSSE
Brooklyn College and
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City University of New York

TED TUNNELL. *Crucible of Reconstruction: War, Radicalism, and Race in Louisiana, 1862–1877.* Baton Rouge: Louisiana State University Press. 1984. Pp. xi, 257. $25.00.

Reconstruction in Louisiana has attracted the attention of numerous historians in recent years. The work under review, which author Ted Tunnell correctly describes as "a series of related interpretive essays" (p. 7), offers a useful contribution to this corpus.

The treatment of wartime reconstruction is essentially an extended critique of certain aspects of the interpretation offered by Joseph G. Tregle and this reviewer. It is a careful analysis, fine-tuning the evidence to present an interpretation critical of Louisiana Radical Thomas J. Durant and favorable toward General Nathaniel P. Banks. His most telling points have already been made by LaWanda Cox in her recent work on Lincoln and Louisiana. Tunnell accepts my delineation of the alignments in the constitutional convention of 1864 and my interpretation of the Civil War as a revolutionary experience. He also seems to agree with Cox and myself that Lincoln was more radical on race and reconstruction than earlier historians have argued.

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Tunnell's collective biography of the constitutional convention of 1867–68 and his careful scrutiny of certain key roll calls add the newest information to our understanding of Louisiana Reconstruction. Following David C. Rankin, Tunnell emphasizes the central role of the antebellum free Negro elite in postwar politics. In contrast to earlier historians, he argues that blacks constituted a slight majority of the delegates and played an independent role in shaping state policy, pushing successfully for more expansive civil rights guarantees. His treatment would have been more illuminating, of course, had he undertaken a more systematic roll call analysis, using simple techniques such as Guttman scaling or cohesion indices.

Like Joe Gray Taylor and Joseph G. Dawson, Tunnell pictures the last decade of Reconstruction in Louisiana as a violent struggle between Democrats and Republicans (in which the Republicans were consistently overpowered by force of arms). Unfortunately, the internecine warfare among Republicans sometimes resembled the politics of a banana republic. His craftsmanlike account adds some new details, especially in his case study of Marshall Harvey Twitchell's experiences in Red River Parish, and strengthens one's sense that the "redeemers" behaved rather like European fascists in our own century.

Tunnell breaks no new interpretive ground, but perhaps that is too much to expect in such well-ploughed fields. He is a careful scholar who writes well and who has added to our fund of knowledge. Specialists will read him appreciatively, even if a wider audience will still prefer earlier works.

PEYTON MCCRARY
University of South Alabama

WALDO E. MARTIN, JR. *The Mind of Frederick Douglass.* Chapel Hill: University of North Carolina Press. 1985. Pp. xii, 333. $27.50.

Frederick Douglass was both a representative American and a representative Afro-American, argues Waldo E. Martin, Jr. The central themes of Douglass's thought were attempts to resolve the tensions in this dual identity. Douglass embraced the ideals of antebellum reformers and used them as weapons in his struggle against the corrosive racism of his times. He championed integration, assimilation, and racial uplift, and, despite a hardheaded appreciation of the obstacles, he remained optimistic.

Martin surveys the contours of Douglass's experience in a four-chapter account of his spectacular ascent from slavery in the backwaters of rural Maryland to service as an abolitionist lecturer and editor, wartime lobbyist for emancipation and black enlistment, and postwar eminence as a racial celebrity and Republican stalwart. Readers familiar with Douglass's autobiographies and the biographies by Benjamin Quarles, Philip S. Foner, and Nathan I. Huggins will find little new in Martin's account, save his valuable discussion of the significance of Douglass's insistence on "black manly independence" (p. 47) in the famous break with William Lloyd Garrison. This was an intellectual and psychological turning point that Martin compares to Douglass's well-known battle against the slave-breaker Edward Covey.

Martin's sustained analysis of Douglass's thought occupies the bulk of the book. In six thematic chapters Martin examines Douglass's views on white racism, feminism, social reform, American nationality, ethnology, and the self-made man. From wide reading in Douglass's published books, articles, and speeches, Martin explores the intricacies of Douglass's ideas. He emphasizes the limitations Douglass's Americanism imposed on his Afro-American identity: his vision of a raceless, composite American nationality devalued Afro-American culture; his reformist politics compromised his revolutionary ideals; his benign view of capitalism blinded him to the "pressing imperative of fundamental economic change" (p. 282); and his Republican loyalties prevented him from developing necessary alternatives to traditional political parties. Although the economic transformations Douglass should have called for and the political alternatives he failed to pursue go unmentioned, Martin's book is the most comprehensive study to date of Douglass's ideas.

In essence, Martin asserts Douglass's representativeness and then criticizes him for it. Although Douglass's ideas may have been common, it is difficult to avoid the conclusion that Douglass's mind—the way he employed his ideas—was uncommon, indeed extraordinary. How many other escaped slaves followed Douglass's footsteps? For that matter, how many white Americans—northerners or southerners—shared Douglass's ideas? If Douglass's mind was nothing more than the representative ideas that filled it, why did Douglass do the extremely unrepresentative things he did? By equating Douglass's mind with his publicly expressed ideas, Martin saps the life from Douglass. Something more than a catalogue of ideas possessed the confident man in the frontispiece portrait, bearded and maned like an Old Testament patriarch. What, remains to be seen.

MICHAEL P. JOHNSON
University of California, Irvine

LOUIS R. HARLAN *et al.*, editors. *The Booker T. Washington Papers.* In thirteen volumes. Urbana: University of Illinois Press. 1972–84. $350.00 the set.

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The appearance of volume 13 of the Booker T. Washington Papers brings to a conclusion this superbly chosen and magnificently edited selection of the writings and correspondence of the celebrated black educator and leader.

It was nearly twenty years ago, at the height of the black revolt of the 1960s, that the National Historical Publications and Records Commission, seeking to enlarge the scope of projects beyond the up-to-date letterpress editions of the complete papers of the founding fathers, asked Louis R. Harlan to prepare an edition of the Booker T. Washington Papers. Substantial subventions came from the commission and the National Endowment for the Humanities, and both the University of Maryland and the University of Illinois Press supplied considerable financial support.

Given both the importance of Washington and the richness of his surviving papers, he was a logical choice as the first black American to receive this kind of scholarly attention. Harlan, already at work on a full-scale biography of the Tuskegeean, was an equally logical choice to conduct the project.

Among the editors of letterpress editions of prominent people, Harlan's approach has been somewhat novel. The first volume consists of Washington's autobiographical writings—the two formal autobiographies as well as other scattered autobiographical materials—together with Harlan's revealing introduction of the way in which these were written. Then, faced with the sheer massiveness of the Washington papers—over one million items at the Library of Congress alone—as well as numerous other documents obtained through a diligent search of relevant manuscripts and archival sources, Harlan opted for a highly selective edition. (Volumes 2 through 13 contain about 1 percent of the total material available.)

Harlan and his co-workers (most notably, Raymond W. Smock, who became co-editor beginning with volume 5) made a judicious selection of Washington's speeches, articles, and correspondence. Washington corresponded with a wide range of black and white Americans, and the incoming and outgoing communications with important leaders of both races are of particular interest. It is important to know that this treasure of materials, in addition to showing us the development of Washington's career, also casts valuable illumination on a wide variety of topics in American history at the turn of the century: politics, race relations, education, economic conditions, philanthropy, and the internal life of the black community.

The material has been selected, edited, and annotated with enormous care and skill. The documents are presented in a strict chronological form, but the particular choices, together with many of the annotations, give us a coherent account of Washington's life and an equally coherent presentation of the many themes and personalities that together were woven into the rich tapestry of Washington's career and influence. The annotations are done thoroughly and meticulously. They keep the reader fully informed without ever becoming tangential essays that detract from the main focus of the work.

The main outlines of Washington's life are by now familiar enough (especially as a result of Harlan's own Pulitzer Prize–winning biography) to render it undesirable for the reviewer to review the conclusions reached by Harlan. One should note, however, that the biography is by no means a substitute for this selection of Washington's papers, which the editors have consciously designed to provide a more profound analysis of the social milieu in which the Tuskegeean operated.

One can recommend this edition of Washington's papers in the warmest terms and without any reservations whatever. It is a tribute to the editors and their staff that this project has been completed within a reasonable length of time (about a dozen years elapsed between the appearance of the first and last volumes). And it is also a tribute to the scholarship of Harlan that, although he has spent nearly a quarter-century virtually "living" with Booker T. Washington, he has maintained his scholarly detachment and his balanced and critical evaluation of the career of this eminent black American. As Allan H. Spear commented in reviewing certain of these volumes, their publication is a milestone for social historians because these papers illuminate so well "not only the life of Booker T. Washington but the several worlds in which he lived."

ELLIOTT RUDWICK
Kent State University

PAUL M. GASTON. *Women of Fair Hope.* (Mercer University Lamar Memorial Lectures, number 25.) Athens: University of Georgia Press. 1984. Pp. xiv, 143. $13.50.

Paul M. Gaston's warm and sympathetic study of Nancy Lewis, Marie Howland, and Marietta Johnson, all residents of the single-tax town of Fairhope, Alabama, foreshadows certain themes sure to inform his work-in-progress on the colony. Though *in medias res* the book stands on its own as a fine piece of women's and social history, for through the lives of these women we begin to see what Fairhope meant as both social experiment and cultural history.

Of the three women, Nancy Lewis, a former slave who lived on colony land, is treated most sketchily and used as a foil to introduce the question of race relations. When forced to confront that issue, these midwestern reformers with Populist leanings reluc-

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tantly adopted the local system of segregation. Gaston treats Howland and Johnson and their reform careers in full. Howland was an author (her *Papa's Own Girl* possibly influenced Edward Bellamy), an ardent cooperator (at Topolobampo and Fairhope), and a feminist. Her philosophy was shaped by American Fourierism, and her eccentricities marked her as the prototypical "independent woman." Gaston slights her love affair with the millionaire socialist Christian B. Hoffman and overlooks the syphilitic decline of her devoted second husband, the publicist Edward Howland. Overall, however, Gaston adds considerably to our knowledge of her career and of the important roles of women within the colony.

With Johnson the idealism of Fairhope emerged on a national scale, for it was she who founded the Organic School devoted to a child-centered curriculum emphasizing personal growth as the end of education. In 1907 she opened her school, in 1909 Joseph Fels gave it major support, and by 1913 there were 150 pupils in attendance—one-third of whom came from the North. Johnson was a major force in propagating the Deweyian notion that "education is growth," and Fairhope's Organic School was a model showcase for her ideas.

Gaston's grandfather was one of the colony's founders, his father was a central figure in its development, and the author attended the famous school. He brings to this study not only personal history but also a keen appreciation for the ambiguities reformers faced in the South. By focusing on these three women, he tells us a great deal about the impulse to experiment, the consequences of such radical action, and the role that women of courage and intelligence played within such a community. Fairhope was an expression of the practical idealism created by the economic uncertainties of the 1890s, a place where individualism and creativity were encouraged, and a small town where it was impossible not to know your neighbors. Gaston has told this story with grace and an appreciation of how such colony efforts have enriched our national history.

ROBERT S. FOGARTY
Antioch Review

KEVIN STARR. *Inventing the Dream: California through the Progressive Era.* (Americans and the California Dream.) New York: Oxford University Press. 1985. Pp. xi, 380. $19.95.

Since 1973 when Kevin Starr published *Americans and the California Dream,* many scholars have been happily anticipating this long-promised sequel. Having regarded the first book as a milestone in California and American cultural history from the time of the Gold Rush to 1915, this reviewer, and perhaps others, expected the second volume to continue the narrative to the present. Instead, Starr surprises us by shifting the focus of his study geographically rather than chronologically, and he dazzles us with his skill in managing this change of scene. Whereas the first volume was almost entirely concerned with that traditional locus of California "high culture," San Francisco and the northern part of the state, the present volume is devoted to Los Angeles and the south. As both a native and a resident San Franciscan, Starr deserves congratulations for transcending the notorious sectional antipathies that have always raged between the two regions and for treating southern California sympathetically and taking the idea of southern California culture seriously.

He uses, to be sure, the concept of culture more in the anthropological than the artistic sense, and snide northerners might argue for the necessity of this approach when dealing with a region reputedly so "culturally barren." This is certainly not Starr's viewpoint. Having immersed himself in an immense volume of literary, semiliterary, and subliterary materials, he rescues from undeserved oblivion scores of pundits, protagonists, and publicists who, he demonstrates, were vital to the shaping of the southern California scene.

And turn-of-the-century southern California was an interesting scene, indeed. Contrary to the stereotype of a cultural wasteland deadened and dominated by sharpers, boosters, and hucksters, Starr shows us a region vibrant with the energies of an extensive colony of the creative. Although most of these regional giants, including the leading avatar Charles Lummis, attained little more than pygmy status on the national scene, it is difficult to avoid the conclusion that they were more often victimized by the stereotype than judged fairly on their merits. This seems particularly true of Lummis, whose devastating commentaries in his periodical, *The Land of Sunshine,* easily invite comparison to Bernard De Voto or Carey McWilliams of a later day.

As Starr also makes clear, these southern California "homers" labored under two related handicaps. They were economic boosters and promoters as well as cultural arbiters of the region, and, as a result, the society that they labored to create was quickly transformed into something else. A semiurban arcadia became a superurban megalopolis, and, like a motion picture "dissolve" (Starr, by the way, has two excellent chapters on Hollywood and the movies), neither observer nor historian could tell exactly when the region ceased being the one and became the other. We owe Starr much for deciphering this socioliterary palimpsest, although his narrative tends to become a kind of lament for a lost Eden. "How hauntingly beautiful, how replete with lost

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possibilities, seems that Southern California of two and three generations ago, now that a dramatically different society has emerged in its place" (p. viii). Because of Starr's labors, we can now understand this era even though we cannot recapture it.

JACKSON K. PUTNAM
California State University, Fullerton

JOSEPH F. KENKEL. *Progressives and Protection: The Search for a Tariff Policy, 1866–1936*. Lanham, Md.: University Press of America. 1983. Pp. vii, 290. Cloth $22.50, paper $11.25.

Jointly researched by J. Richard Snyder and Joseph F. Kenkel, then written by Kenkel, this book gives its primary attention to the efforts of "businessmen and politicians, farmers and industrial workers' representatives, and others of a progressive mind" to develop "a stable and reasonable tariff policy . . . so as to promote the common good." In doing so, they sought those quintessential goals of the Progressive faith: a public policy that would be "business-like, nonpartisan, and scientific" (p. 1). They failed—hardly a surprising outcome in light of the complexities the tariff presented to anyone who attempted to change it. Still, those efforts at change make for a revealing episode in Progressivism and American politics, and the labors did produce some results. Most important, American tariff policy attained new flexibility by the 1930s.

Although Kenkel begins his story with the Revenue Commission of 1866 and ends with the Reciprocal Trade Agreements Act of 1934, he pays most attention to tariff policy in the first three decades of this century. Generally, he finds inertia. American tariffs were high and generally remained that way. Many had a notion about what "business-like" meant, but no one could define it or implement it in a broadly acceptable way. Politicians kept politics in tariff policy because it was expedient to do so. Determining whether import duties were excessively or inadequately protective defied the experts of the Tariff Board and two tariff commissions. By the 1920s, some Progressives wanted to dissolve the United States Tariff Commission (created in 1916) because they believed it had become a captive of the rigid protectionists. They had similar designs against the Federal Trade Commission, another product of the lofty goals of Progressivism.

Inertia had other causes, of course. Most important, as Kenkel notes, was the overwhelming emphasis American politicians and businessmen gave to the domestic market. Even after many of them were apparently convinced that the United States needed expanded overseas markets, they were not willing to risk the domestic market in order to expand foreign trade. Nor did the shock of the Great Depression change this attitude. Congress did give the president greater flexibility in tariff policy in the 1920s and 1930s, but the willingness to risk the domestic market awaited another day.

Kenkel fails, however, to note other important factors that contributed to the inertia on tariff reform. Before the imposition of higher federal taxes on personal incomes during World War I, custom duties were the major source of federal revenue. Moreover, with few favors to dispense to voters before the era of burgeoning federal budgets, congressmen and senators could use tariff votes to favor constituents.

Kenkel probably would have written a stronger monograph if he had done several things. In his rather extensive research he ought to have consulted more recent scholarly work. Also, some roll call analyses of Congress would have been enlightening. Last, he should have related the tariff more consistently to broader historical contexts. Deflationary and inflationary trends, long-term developments in agriculture and manufacturing, struggles over monetary policy, and the recurrence of antimonopolism, for instance, were critical to the times and the tariff policy he wrote about. Despite these shortcomings, however, Kenkel's (and Snyder's) efforts have resulted in a useful coda for Frank W. Taussig's aged standard, *The Tariff History of the United States* (1931).

THOMAS E. TERRILL
University of South Carolina

ROBERT W. CHERNY. *A Righteous Cause: The Life of William Jennings Bryan*. Edited by OSCAR HANDLIN. (Library of American Biography.) Boston: Little, Brown. 1985. Pp. xi, 225. $15.95.

Any effort to understand William Jennings Bryan has to overcome the obstacle of William Jennings Bryan himself. His personal and intellectual style, his lack of introspection, the dearth of personal revelations he left behind, his compulsive optimism, his devotion to people in the mass rather than to people as individuals, his ability to rationalize all events so that they confirmed his expectations and the fundamentals of his faith—all this makes it exceedingly hard to probe beneath the surface and find Bryan the human being. A biographer has to struggle to overcome these formidable difficulties.

No less formidable is the task of adequately portraying Bryan's tumultuous career. Bryan lived a dynamic life filled with exciting peaks; he was perpetually stirring up things and people. Robert W. Cherny occasionally does justice to these aspects of Bryan's career, as in his account of Bryan's "Cross of Gold" speech, but for the most part he has eschewed

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drama and written a careful, sober biography. He is particularly good at re-creating the political and economic context for Bryan's early career in Illinois and Nebraska, but Bryan himself remains largely a one-dimensional figure. Consequently, we often understand the milieu in which Bryan operated better than we understand Bryan himself.

Toward the end of the volume this rich, informative depiction of the context of Bryan's politics breaks down, and we are never adequately prepared for his apparent transition from being an apostle of reform to being a conservative guardian of the cultural past. When, during the Scopes trial, Clarence Darrow asked Bryan if the world was made in six days, Bryan replied, "Not six days of twenty-four hours." Cherny describes the "gasp of the startled Fundamentalists" at this heresy (p. 180). Cherny's readers will be no less startled, since he never prepares us for this complexity in Bryan's version of the true faith. Indeed, a number of the complexities in Bryan's life get lost in this fast-moving account.

Those of us who recall the sad state of Bryan scholarship a quarter of a century ago will not lightly dismiss the virtues of a succinct, judicious biography. Cherny has given us a useful, well-written book that draws on both original sources and the many excellent studies of Bryan that have emerged in recent years. Is it fair to ask more than this? I think so. Years ago when I was writing on Afro-American humor, a friend commented that to be wholly successful a study of humor should itself be funny. Similarly, the biography of a dramatic, dynamic, complex life should itself be rich in drama and complexity. Unfortunately, too many dramatic, dynamic, complex strands slipped through Cherny's fingers as he crafted this biography, which is truer to the substance than the spirit of its subject's life.

LAWRENCE W. LEVINE
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HOWARD P. SEGAL. *Technological Utopianism in American Culture*. Chicago: University of Chicago Press. 1985. Pp. x, 301. Cloth $30.00, paper $14.95.

In *The Machine and the Garden* (1964) Leo Marx analyzed the widespread American desire for a divorce between technology and society; Howard P. Segal believes that many sought an accommodation between the two. He launches his ambitious, scholarly argument by discussing twenty-five technologically based utopian novels written between 1883 and 1933, from just before the publication of Edward Bellamy's *Looking Backward* (1888) to the brief fad of technocracy. He also examines the European and American origins of this form of utopian thought and the ideas and activities of other nonutopian American intellectuals and reformers whose work considered the social meanings of technology. Sympathetic to his subjects, Segal believes that utopian writing can be a useful form of social criticism, but he sees many shortcomings in writers who placed their faith in technological revolution rather than in changes in social relations.

Segal writes that technological utopians were less nostalgic and more practical than other utopian writers. They believed that with correct application technology would turn the impossibly perfect blueprint into the probable social outcome. Their curiously conservative role was not to escape the real world but to play back their vision in order to change the real world.

In such utopias both technology and nature would be tamed through material, mechanical processes. Efficiency was both the cleansing means and the desired social outcome. Efficient rulership would employ strictly technical and bureaucratic methods; politics, that discordant means of arbitrating tensions in society, would ultimately wither away. The ideal personality in this harmonious final place would be one of complete self-control, the "machine-like virtue" in Segal's chilling phrase (p. 31).

In one of the most creative chapters in this study, Segal compares this form of utopianism with other technologically centered piecemeal reform movements such as the Tennessee Valley Authority, world's fairs, scientific management, national planning centers, and research foundations. At first glance these have little in common, but Segal shows that liberal, elitist reform existed on a continuum with the more extreme technological utopian vision. Furthermore, he believes that, because American society has tended to move toward the ends predicted by these utopians, technological utopians have played a significant role in this country's critical social thought.

Segal stresses the practicality of his utopian subjects, perhaps in an attempt to emphasize their modernity and social realism. In so doing, he downplays the centrality of perfectionism, the key emotional and intellectual element in their thought. Their goal was a world freed from the morass of social contradictions and individual drives. The technocratic "anti-politics" Segal mentions was one expression of these writers' compelling need for order. Segal might well have extended that discussion, as well as his brief psychological analysis of his subjects' obsessions with completion and stillness. To what degree were the technological utopians rejecting both their own humanity and that of others in their effort to remake the world into a perfect machine? Why was their writing about the transition between this world and the next so unconvincing? In his wide-ranging footnotes and

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superb bibliography, Segal has provided the interested reader with many avenues to explore these and many other disturbing questions raised in this thoughtful book.

MICHAEL FELLMAN
Simon Fraser University

GARY SCOTT SMITH. *The Seeds of Secularization: Calvinism, Culture, and Pluralism in America, 1870–1915*. St. Paul, Minn.: Christian University Press or Wm. B. Eerdmans, Grand Rapids, Mich. 1985. Pp. viii, 239. $14.95.

One benefit of what Henry F. May called the "recovery of American religious history" is that we have thereby gotten rid of the notion, formerly widespread in our guild, that religion must be studied from a nonreligious, or at least nonsectarian, perspective if the study is to be considered scholarly or "objective." The work reviewed here is an insider's book; it was written by a committed Calvinist, directed as a doctoral dissertation by an equally dedicated heir (Timothy L. Smith) of a somewhat different theological tradition, and intended in part as a contribution to inter- and intrasectarian debate. Yet it is at the same time a valuable correction of a favorite conceit of American cultural history; namely, that Calvinism in America was a spent force by the time of the Civil War.

William Warren Sweet, who launched the academic study of American church history, gave a classic statement of that thesis in his last, and probably best, book, *Religion in the Development of American Culture* (1952). After dealing with the "nationalizing" of formerly Europe-centered churches, Sweet turned to a subject dear to his Methodist heart, namely, the revolt against Calvinism. Historians of religion in America have pretty much followed his cue, accepting the elder O. W. Holmes's dictum (symbolized by his poetic metaphor of the wonderful one-horse shay) that Calvinist theology held together for a hundred years and a day and then collapsed in a cloud of dust. Not so, Gary Scott Smith warns; Calvinists in America at the turn of the century were still, both numerically and intellectually, a formidable cultural force. Moreover, their dissent from the onrushing tides of secularism was the most coherent and convincing rebuttal available; more so (by implication) even than that of Catholicism.

Where the Calvinists went astray, this book argues, was not in the inadequacy of their arguments against scientific humanism but rather in their efforts at cultural and even political hegemony, forgetful that their own assumption of the innate depravity of humankind made the hoped-for "Christian America" a logical and theological impossibility. What they ought to have worked for, the author contends, was "confessional pluralism" on the Dutch or Canadian model, with distinct religiocultural communities in political coexistence—including, should they desire so to organize themselves, the secular humanists.

One distinguished foreign visitor, the Dutch theologian (and sometime prime minister) Abraham Kuyper, argued that such a system was working well in his native Netherlands. Unfortunately, in Smith's view, most American Calvinists never seriously considered cultural pluralism as an option; like other WASPs, they mistrusted both the unassimilated immigrants and their ancient Catholic adversaries too much to envision living in pluralistic harmony with them.

One need not accept Smith's highly specific intellectual and confessional premises to appreciate the force of this argument. The documentation is careful and thorough; the thesis is presented with true Presbyterian logic. "Secular humanists," if they do not mind that epithet, can profit as much from reading this book as anybody else.

PAUL A. CARTER
University of Arizona

LYNN DUMENIL. *Freemasonry and American Culture, 1880–1930*. Princeton: Princeton University Press. 1984. Pp. xviii, 305. $30.00.

A number of historians have profitably examined the nature of American Freemasonry in the early republic, when it served as a target of anti-elitist protest and a symbol of social disintegration, but few have carried the investigation of the secret fraternal order past the Civil War, when it became respectable and popular. Lynn Dumenil's sensitive, thoughtful study repairs that neglect by analyzing the ideology and appeal of Masonry during the period that marked the emergence of modern American consumer culture.

Drawing on a nationwide sample of Masonic publications and on the records of the Live Oak Lodge in Oakland, California, Dumenil describes the ways in which Masonry in the late nineteenth century enabled the primarily native, white, middle-class men who comprised its membership to achieve a sense of community and affirm traditional values. Citing Masons' repeated references to the lodge as an "asylum," Dumenil argues that between 1880 and 1917 the order provided a refuge from commercialism, conflict, and the dislocations accompanying urbanization. In particular, as she deftly demonstrates, the multiple religious views Masonry espoused accommodated tensions within contemporary Protestantism, while the rituals it practiced permitted the expression of both spirituality and

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solidarity. After World War I, however, Masons abandoned "resistance" to change in favor of "adaptation" to an organized, secular society (p. 221). Ritual lost ground to crusades on behalf of "Americanism"; an emphasis on public service superseded the lodge's former preoccupation with personal morality. That transformation, Dumenil concludes, mirrored the shift in values that accompanied modernization in America generally.

Dumenil's effort to explicate Masonic rhetoric and to connect it to larger cultural trends is imaginative and intelligent. Yet her distinction between nineteenth- and twentieth-century Masonry seems overdrawn. On the one hand, the "asylum" image notwithstanding, Masons in the 1880s and 1890s, like their counterparts in the 1920s, used the lodge for quite worldly purposes: to cement business relationships and to acquire social prestige. Moreover, the prominence of Masons in the leadership of the anti-Catholic agitation of the 1890s foreshadowed rather than contradicted the political posture of the order thirty years later. On the other hand, to make "adaptation" to modern conditions the distinguishing feature of twentieth-century Masonry is to obscure what Dumenil herself recognizes: that Masonic xenophobia proceeded not from an embrace of the present but from a desire to reclaim the past by restoring the waning authority of the white Anglo-Saxon Protestant.

The book does not supply much information, except for occupational data, about the lives of the lodge's members, especially those outside of Oakland. A fuller understanding of Masonry's attraction would require not only a sample of more localities but also an exploration, insofar as possible, of the relationship between Masonic affiliation and such matters as family structure, political power, education, ethnicity, and religion within a local context. Nor does Dumenil treat in any detail the extent to which Masonry resembled other American fraternal groups. Yet she frankly acknowledges that a great deal of comparative work remains to be done. Scholars undertaking that work will find themselves considerably in her debt.

JOAN SHELLEY RUBIN
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RICHARD FRANKLIN BENSEL. *Sectionalism and American Political Development, 1880–1980*. Madison: University of Wisconsin Press. 1984. Pp. xx, 494. $35.00.

In this sweeping reinterpretation of American political development, Richard Franklin Bensel argues that "sectional competition—grounded in a geographical division of labor between the economically advanced northern core and the underdeveloped southern and western periphery—has been and remains the dominant influence on the American political system" (p. xix). Adapting his core-periphery analytical framework from Immanuel Wallerstein's world-economy theory, Bensel also analyzes the country's progress "from an agrarian, periphery economy" in the world economy "to a dominant industrial power at the center of the global core" (p. 407). This ambitious and bold venture involved a prodigious amount of work, and Bensel's analysis is constant, provocative, and often compelling.

The primary data base is constructed from roll call behavior in the House of Representatives—the best available reflection, Bensel argues, of the interregional struggle for control of the national political economy. To secure a timely indication of economic interests and interdependencies in the ten decades since 1880, he divides the nation into regional "trade areas," each with a dominant urban center and a dependent "hinterland" of rural areas and subordinate cities. After placing congressmen in their respective trade-area delegations, Bensel calculates "sectional stress scores" on all competitive congressional roll calls in the middle of each decade. He centers his analysis on the ten roll calls having the highest sectional stress scores, but he frequently extends his consideration into preceding and subsequent Congresses.

Bensel first concentrates on the tariff, military pensions, contested elections in the South, and imperialism issues; the predominantly Republican trade delegations from the northeastern industrial core generally aligned with the agrarian Midwest against the Democratic South. This alignment began to unravel as the agrarian periphery opposed the stand of the industrial core on military preparedness before World War I and as the Midwest allied with the South on McNary-Haugenism and other issues during the 1920s. Sectional conflict lessened in the face of economic distress and the emergence of the "bipolar Democratic coalition" between laboring classes in the industrial core and Democrats from the southern periphery during the 1930s. Here and in later chapters, Bensel gives extensive attention to institutional changes such as the decentralization of power into the congressional committee system. Finally, he details the sectional competition over restrictive economic controls after World War II, agricultural supports and controls during the 1950s, and civil rights. Despite the removal of civil rights as an intensely sectional issue, disputes between the core and periphery continued into the 1970s on a myriad of problems stemming from the energy crisis, the decline of heavy industry in the core, and the economic development of the periphery.

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On these issues Bensel finds a "persistent, unchanging pattern of sectional alignment" that has "decisively shaped the institutional structures, political parties, and ideological belief-systems of American political life" over the past century (p. 411). With the disintegration of the New Deal coalition and the weakening of the congressional committee system, Bensel tentatively suggests that the shift of the Democrats into the formerly Republican-dominated industrial core and the expansion of the Republicans into the southern and western periphery may signal a sectional repolarization of the congressional party system.

Those who favor cultural, ideological, consensus, or Marxist interpretations will dispute some of Bensel's arguments and conclusions, and others will take exception to his methodology. Nonetheless, this remains a noteworthy study that establishes sectional competition as a primary factor in American political development. No student of regional or national politics since Reconstruction should fail to give it a close reading.

TERRY L. SEIP
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JON C. TEAFORD. *The Unheralded Triumph: City Government in America, 1870–1900.* (Johns Hopkins University Studies in Historical and Political Science.) Baltimore: Johns Hopkins University Press. 1984. Pp. xi, 365. Cloth $28.50, paper $14.95.

James Bryce's famous pronouncement—that the governing of the cities was the "one conspicuous failure" of the American political system—has been endlessly misunderstood. *Unheralded Triumph,* intended as a riposte to Bryce's condemnation, misunderstands him at book length. Jon C. Teaford begins his labors with an interesting hypothesis, but his desire to be fashionably perverse in presenting his findings misdirects the book as a piece of historical scholarship. Even the good he might have done as devil's advocate is lost, for Teaford cavalierly ignores virtually all recent writing on late nineteenth-century city government, most of which supports Bryce's assessment.

Bryce considered city governance a failure in comparison with the success of the American political system as a whole. His criterion was the "adaptability of the institutions to the people and their conditions" (*American Commonwealth* [1889], vol. 1, p. 618), and by his lights the city governments of the 1880s fell far short. Teaford contends that Bryce was wrong, that, in fact, the "formal, legal structure" (p. 3) of city governments in the period 1870–1900 coped with the demands of diverse, growing populations "with remarkable success" (p. 6). "Late-nineteenth-century urban government was a failure not of structure but of image," Teaford protests (p. 10).

The structural arrangement most vigorously condemned was state legislative interference in city governance. Using *American Commonwealth* as the basis for a new municipal political science, Bryce's American followers defined the "municipal problem" of large cities as a lack of strong powers of self-government. Squaring off against these reformers of the 1890s, Teaford hypothesizes that state legislative control offered "structural flexibility" in meeting the expanding needs of city populations (p. 8). He explains that, "though state legislatures were potentially masters of the city, through much of the United States they were actually compliant servants of urban spokesmen" (p. 84).

This would be an interesting state of affairs if it were true, especially in the current era of Nixonite and Reaganite "new federalism." It surely deserves to be carefully evaluated. All Teaford offers us, however, is a hodgepodge of examples mediated by strong caveats. He does not believe his claim applies to New York City and New York State. Nor does it hold when the state legislature and city government are controlled by different political parties. He assures us that differing party control occurred "only occasionally" (p. 98), but to provide quantitative evaluation of this assertion would have been a simple matter.

The majority of Teaford's text is an impressionistic amassing of urban improvements: sewers, water systems, health departments, libraries, electrified streetcars, enlightened social services, and professionalized teachers, police, and firefighters. Teaford does not attempt to determine systematically whether enough sewers, libraries, and health inspectors were provided to meet the population's needs. We know that city government expenditures per capita for all large cities declined by 12 percent between 1880 and 1904 (8 percent in constant dollars), not because cities were not trying harder but because immigration and rural-to-city internal migration were swelling their populations at an astonishing rate.

Teaford may be right that state legislative control was a workable structural arrangement, but does it meet Bryce's standard for good government: the adaptation of governmental institutions best suited to the needs and desires of city populations? Teaford judiciously avoids claiming that cities welcomed state legislative control, and he does not adequately explain that its strongest proponents were rural legislators who hated and feared the city's power. City interests may have learned to manipulate the state legislature successfully, but few regarded it as the proper way to govern a city.

KENNETH FOX
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H. LEON PRATHER, SR. *We Have Taken a City: Wilmington Racial Massacre and Coup of 1898*. Cranbury, N.J.: Associated University Presses. 1984. Pp. 214. $19.50.

This exemplary monograph on the racial massacre and the coup of 1898 in Wilmington, North Carolina, contains all the elements of a tragic drama. In that year a Mrs. Felton addressed an agricultural society meeting in Tybee, Georgia, stating that "if it needs lynching to protect woman's dearest possession from the ravening human beasts—then I say lynch, a thousand times a week if necessary" (p. 71). Alex Manly, the successful black editor of the Negro paper the *Record*, took issue with the classic southern rape fantasy in a scathing editorial. Reactions from the white community varied: Josephus Daniels, owner and editor of the *Raleigh News and Observer* and later Woodrow Wilson's secretary of the navy, used words, but others reacted with ropes and guns. Lynch they did, but also massacre, maim, burn, and steal; the whites were the culprits, the blacks their hapless victims.

The crimes of blacks were to be Republican and financially successful, to hold municipal offices such as coroner and city auditor, to create an all black prizewinning fire brigade, to serve on the police force and as justices of the peace, and to comprise some 30 percent of the aldermen of the town. Black tradesmen were also evident and owned drugstores, groceries, and pawnshops frequented by whites. Other blacks worked in the heavy industries, cotton presses, seaports, shipyards, and warehouses. From an apparently model, integrated community of eleven thousand blacks and eight thousand whites, Wilmington descended into a nightmare of intrigue, bloodshed, and mayhem.

H. Leon Prather, Sr., skillfully documents the reasons. Latent race hatred, unemployment among poor whites, and political rivalry were like dry timber waiting for a spark. Prather insists the riot "did not erupt spontaneously, accidentally, or in a vacuum" (p. 183), and with the craftsmanship of a good dramatist he allows the tragedy to unfold.

The Democrats lost the mayoral election of 1897 and the subsequent state Supreme Court case questioning the constitutionality of the election. The Democrats were desperate and pledged clandestine allegiance to a citywide protective campaign that was coordinated with the statewide white supremacy movement. Organizations like the Secret Nine, the Redshirts, and the Rough Riders took the law into their own hands. Some wanted to hunt blacks in the open; others preferred secret conspiracy. The result was the burning of Manly's newspaper offices, the massacre, and, finally, the coups that overthrew the duly elected Republican government.

The Progressive era was the nadir of race relations, marked by mob violence, lynchings, segregation ordinances, and the landmark Supreme Court case, *Plessy* v. *Ferguson*. Prather adds the details of the Wilmington massacre to the Progressive legacy, correcting an important historical omission.

There are many powerful quotations in the book documenting the Negro's plight during this period. One anonymous observer of the riot, a black woman, pleaded with William McKinley: "Are we to die like rats in a trap? . . . Why do you forsake the Negro who is not to blame for [our] being here? Is this land of the free and the home of the brave? How can the Negro sing My Country tis of thee?" (p. 157). McKinley was silent. The armed insurrectionists were not prosecuted.

There is no final curtain to this play, for Prather's epilogue cites the ongoing nature of racial disturbance, including the "Wilmington 10," which emerged out of the turmoil of the 1970s, much to the Carter administration's embarrassment. With painstaking scholarship and a clear humanitarian position, Prather has written a book to excite the general public as well as the scholar.

NANCY WEISS
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KATHLEEN BRADY. *Ida Tarbell: Portrait of a Muckraker*. New York: Seaview/Putnam. 1984. Pp. 286. $17.95.

Ida Tarbell is one of the few American women who receive a sentence or two in American history textbooks. Surprisingly, this is the first biography of her to be published. Best known for her muckraking *History of Standard Oil Company* (1904), Tarbell also wrote biographies of Abraham Lincoln and Napoleon, as well as more than a dozen other books and hundreds of articles. She made her living as a writer, investigative reporter, lecturer, and editor at a time when such a career was unusual for a woman. She was also a strong-willed executive who helped to make *McClure's* one of the leading American magazines in the late 1890s. For the decade after 1906 she was an editor and one of the co-owners of *The American*, one of the best of the new crusading magazines.

Born near Titusville, Pennsylvania, in 1857, she graduated with an A. B. from Allegheny College in 1880. Like so many others of the first generation of college women, she never married and floundered around for a number of years before finding a suitable career. Originally interested in biology, she taught high school for a time before joining the staff of the *Chautauquan*. In 1891, after resigning or being fired, she moved to Paris to write the story of Mme. Roland. Joining forces with three other American women, she lived in a tiny apartment and

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supported herself by writing descriptive articles about Paris for American newspapers. She also sold a story to *Scribner's*. But her big break came when Samuel McClure, a young, flamboyant American editor, looked her up in Paris and offered her a job. At first she turned him down, but the next year when she returned to America she accepted. It was a strange and stormy match but one that was profitable to both McClure and Tarbell. When her articles on Napoleon began to appear, the circulation of *McClure's* more than doubled.

Tarbell went from one triumph to another to become one of the most successful and best paid writers and lecturers of her day. She traveled widely and knew many of the rich and famous people in both the United States and Europe. Yet she was never entirely happy, and in a book published in 1912 she argued that women's traditional roles of wife and mother were more important than public life. Even though her own life seemed the model of what an independent woman could achieve, she opposed women's suffrage and alienated many of the feminists of her day. Most famous for attacking the business practices of Standard Oil, she spent much of her later career defending American business, especially Frederick Taylor's scientific management and Henry Ford's industrial paternalism.

This biography explores some of the contradictions and enigmas in the life of a fascinating and important person. Written by a novelist and freelancer, it is based on thorough research in manuscript collections and on a knowledge of most of the relevant secondary literature. Occasionally it lacks perspective, and the characterizations of some of Tarbell's contemporaries are oddly skewed. But it is a well-written and lively book. It should be read by all those interested in the history of journalism and the Progressive era as well as by those concerned with the role of women in American history.

ALLEN F. DAVIS
Temple University

LINDA KILLEN. *The Russian Bureau: A Case Study in Wilsonian Diplomacy*. Lexington: University Press of Kentucky. 1983. Pp. xii, 202. $22.00.

Linda Killen's book will be welcomed by specialists on Soviet-American relations, for it skillfully probes an unexplored dimension of the American reponse to the revolutionary situation in Russia in 1918–20. Killen focuses on the War Trade Board's U.S. Russian Bureau, which she views as Wilson's primary vehicle to assist the Russian people bring about political and economic recovery and stability. Wilson typically limited the activities of the bureau to those areas of Russia controlled by supporters of the March revolution rather than those areas controlled by the Bolsheviks.

Killen carefully evaluates the origins of the Russian Bureau, the nature of its activities in Russia, and the results that occurred when the bureau was phased out in 1919. Wilson's concept of aid is placed in the proper context of establishing normal free enterpise trade relations rather than the context of post–World War II versions of grants and assistance. Killen reviews the maneuvering between the Commerce and State departments for control of economic relations with Russia. Wilson belatedly decided in October 1918 to give the War Trade Board authorization to set up the Russian Bureau to license private corporations to export goods to Siberia and other areas; for this purpose the bureau was given five million dollars. Within one month after its creation the Russian Bureau, which had offices in Washington, Archangel, and Vladivostok, got ensnared in the crosscurrents of Allied rivalry, transportation difficulties, a lack of shipping for exports to Siberia, and uncertainties after the end of the war.

Killen candidly admits that the bureau accomplished little in promoting trade, that it provided little aid, and that its funds were ultimately transferred to an interallied railway fund for the Trans-Siberian Railroad. Responsibility for the Russian Bureau's failures is judiciously attributed to the turmoil in Russia, unrealistic expectations in Washington, and Wilson's reservations, especially his opposition to spending government funds on an assistance program such as the Russian Bureau's proposal to aid Russian cooperatives.

The brief existence and meager results of the Russian Bureau raise several questions about this study. First, Killen thoroughly researched personal papers and United States government records but omitted newspapers and periodicals. The failure to consider business opinion concerning the Russian market and the bureau—especially the opinion of the American Russian Chamber of Commerce, mentioned in the text twice and in one footnote—is a serious omission. The chamber was active in this period, and its papers are available for research. Second, in the conclusion Killen ably reviews not only the failures of the bureau but also the shortcomings in Wilson's and his advisers' concepts and handling of the bureau. Yet about half of the concluding chapter contains interesting speculations about Wilson and Russia that seem to come more from Killen's dissertation than from this book. Perhaps the publisher, not the author, should be criticized for having published this book, which could be a compact article, instead of her earlier, more comprehensive study of Wilson's Russian policy.

THOMAS R. MADDUX
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ROY V. SCOTT. *Railroad Development Programs in the Twentieth Century.* (Henry A. Wallace Series on Agricultural History and Rural Studies.) Ames: Iowa State University Press. 1985. Pp. xi, 231. $22.50.

The promotional activities of railroads in the late nineteenth century are fairly well known to American economic historians. The work of railroads in encouraging economic development in the twentieth century, however, is not a familiar story. Consequently, Roy V. Scott has made a distinct contribution by showing how twentieth-century railroad companies developed and implemented programs designed to encourage general economic development in their trade areas and to increase traffic on their lines.

Railroad officials saw that economic growth was necessary for new company business. In the early years of the twentieth century they concentrated on expanding agricultural traffic. In the decade before World War I most major railroads established agricultural departments and hired specialists to improve the farm business. These agents promoted settlement in the West through advertisements and publications, supported irrigation, ran excursion trains, and encouraged immigration. The agricultural departments of the railroads also worked to promote better farming practices. Some lines established demonstration farms, ran demonstration trains, promoted diversification, distributed literature on better farming, and supported all kinds of practical agricultural education. After 1930 the effort by railroads to promote agricultural improvement declined as other agencies such as the Cooperative Extension played a larger role.

Meanwhile, railroads pushed for industrial growth as a means of increasing traffic. They sought to attract businesses and industries to their service areas by advertising, offering company-owned real estate to prospective industrialists at favorable prices, organizing industrial parks, and providing other encouragements to businessmen. Scott explains that the railroads were also active in developing mineral and forest resources. After World War II most of the railroads' work in the field of economic development dealt with the promoting industry.

Scott has written a clear, straightforward account of efforts by railroads to promote economic development from the turn of the century to the 1970s. He describes the motives, organization, and implementation of plans for economic expansion. The book is based on a wide selection of both primary and secondary sources.

Scott is generally sympathetic to the efforts of the railroads, a position that would probably disturb the late Fred A. Shannon, his former professor and railroad critic. This study probably will interest a rather narrow audience, but that has more to do with the subject than with the quality of the book. The Henry A. Wallace Series at Iowa State University has published another splendid volume.

GILBERT C. FITE
University of Georgia

GRAHAM D. TAYLOR and PATRICIA E. SUDNIK. *Du Pont and the International Chemical Industry.* (Evolution of American Business, Industries, Institutions, and Entrepreneurs Series.) Boston: Twayne. 1984. Pp. xxi, 250. $18.95.

Graham D. Taylor and Patricia E. Sudnik have written a comprehensive and readable study of the E. I. Du Pont de Nemours Company and its international relations. The study covers the company from its beginnings through the late 1970s. Drawing extensively on the Du Pont papers held in the archives of the Eleutherian Mills Historical Library, on interviews with former company employees, and on many other sources, the authors begin with an account of the "transformation of the chemical industry, 1780–1910." The first chapter, though introductory, is useful because of its succinct summary of European (and, less extensively, American) developments in chemical technology in this period.

The second chapter focuses on Du Pont's beginnings through 1913 and is, again, a useful, concise account. Established as a powder company by Eleuthere Irenee Du Pont in 1801, Du Pont was producing, by the time of the Civil War, one-third of the total explosives output in the U.S.; by 1900 it had become the largest explosives manufacturer in the country. In 1902 the company, having grown through joint ventures and mergers, was reorganized by three Du Pont cousins—Pierre, Alfred, and Coleman. The authors emphasize throughout the book the unusual continuity of management and control of the firm by family. Also, they focus on Du Pont's strategy of growth by merger.

Moving from being primarily an explosives company, Du Pont underwent phenomenal diversification, a process given major impetus by the antitrust suit of 1907–13, which split the huge powder company into three smaller units: Hercules, Atlas, and Du Pont. Diversification was also stimulated by World War I and by the dyestuffs industry, which supplied important chemicals for the manufacture of explosives. These chemicals also ultimately served as a linkage to other kinds of products such as synthetic fibers. Chapter 4, "The Impact of War, 1914–1918," contains useful information on other chemical firms, especially American Cyanamid, and the early commercial ventures in the fixation of atmospheric nitrogen.

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The book charts Du Pont, with all its internal struggles, through periods of tremendous growth during the world wars and 1920s, through the Great Depression (which the company weathered better than most), through antitrust litigation and public outcry against the "merchants of death," and, more recently, through a period of denunciation by environmentalists. The authors cover well the evolution of the myriad of Du Pont products, both those, like nylon, innovated by Du Pont researchers, especially after 1930, and those acquired through merger. Also, the story of Du Pont investment in General Motors is told here.

The focus of the book is on Du Pont's international relations and its strategies in foreign markets, especially the pursuit of patent agreements with firms such as Imperial Chemical Industries of Britain, with which Du Pont established particularly close ties. As the authors assert, "Following World War II, Du Pont dominated the international chemical scene, the virtual embodiment of American entrepreneurial and managerial capabilities" (p. 197). Sudnik's research on Du Pont's foreign activities is clearly a real contribution to knowledge.

The book is well researched and written, and Albro Martin should also be commended for this addition to the series he edits. Despite one or two typographical errors and a sexist example that opens the first paragraph of the first chapter, the book represents a much-needed contribution to the historiography of the chemical industry.

MARTHA M. TRESCOTT
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CHARLES W. CHEAPE. *Family Firm to Modern Multinational: Norton Company, a New England Enterprise.* (Harvard Studies in Business History, number 36). Cambridge: Harvard University Press. 1985. Pp. xii, 424. $25.00.

This book by Charles W. Cheape is the history of the Norton Company, a little-known manufacturer that employs over twenty-five thousand people in 120 plants located in twenty-eight countries, producing a wide range of metal-working, petroleum-drilling, and mining equipment, as well as related industrial products.

Originally formed by seven partners in 1885, the firm produced cutting and grinding wheels composed of metallic particles bonded with ceramic products. Producing wheels suitable for a multiplicity of functions in building the increasingly sophisticated machinery of the technological revolution required extensive experimentation, which the artisan partners conducted on a trial-and-error basis. But the burgeoning demand for such implements in the growing industrial center of Worcester, Massachusetts, provided an increasing and ever-changing market for new types of abrasive implements and materials. This company increased rapidly in size and in the diversity of its products. By 1893 it had returned over 750 percent of the original $20,000 investment.

Marketing was conducted by a small traveling sales force. Executive functions were carried on informally by groups of partners led by Charles Allen. Familial relationships of the original partners were reflected in paternal relations with employees. In addition to offering stock options, the company built homes and recreational facilities for their workers. The entry of unions was successfully opposed.

As Charles Allen later recalled, "It was when the wonderful automobile came down the pike that grinding got its great impetus" (p. 58). By World War I the Norton Company had become a big business, but it still continued to be a family operation. Bureaucratic structure was established, and, as other firms entered the business, an industrial association came into being to give price stability and product standardization. Foreign sales formerly carried on by traveling members of the family was succeeded by the acquisition or building of plants in Germany, France, and Japan.

World War II and the subsequent European depression brought problems in relationships with foreign managers, but the business soon bounced back to prosperity after the war. The aging second and third generations lost power, however, as the business was rationalized and decentralized with the formation of a General Development Committee and a foreign operations holding company. Owner operation at the top gradually disappeared between 1960 and 1980, when Norton went public and offered its shares on the New York Stock Exchange.

Since the author of this volume had unrestricted access to company records and conferred at length with active and retired personnel of the company, the book is comprehensive and replete with pertinent detail, particularly in the development of machine tools—an area sometimes given scant attention in business history. The book also contains vivid descriptions of the individuals concerned and the effect of their personalities on the conduct of the business. It is a well-written and valuable addition to the history of American business because of the complete picture it gives of the growth of a local artisan workshop into a multinational enterprise.

ELISHA P. DOUGLASS
University of North Carolina

ROGER M. OLIEN and DIANA DAVIDS OLIEN. *Wildcatters: Texas Independent Oilmen.* Austin: Texas Monthly. 1984. Pp. xi, 234. $16.95.

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This book explores the historical role of independent oilmen in the discovery and production of crude oil from 1920 to the 1980s, focusing on the Permian Basin of western Texas and the shifting fortunes of Midland, its oil capital. As Roger M. Olien and Diana Davids Olien point out, "independent" has had varied meanings over time. For their purposes, the typical independent operation is not integrated; it is less managerially organized and more limited as to areas of operation and access to capital than the so-called major oil companies. Independents may be of varying sizes, with some large and a few integrated; some are of intermediate size, although the line between them and the others is not sharply defined. Most typical are the thousands of small operators who flourish or disappear depending on the fortunes of the oil industry.

A substantial part of the book deals with the industry's changing environment that either encouraged or discouraged independents, who over time had to respond to fears of shortage, oversupply, imports, federal tax policy, and federal and state regulation. Within this context independents reflected the state of the market, geological and engineering knowledge, and problems of access to capital and transportation from the oil field. Contrary to popular belief, in all of these areas there was often more cooperation than conflict between independents and the majors.

A landmark of oil history was the discovery of the great east Texas field in 1930 as the nation was entering a deep, prolonged depression. The problem of dealing with flush production in that period of falling prices led to both state and federal regulations to prevent physical waste. The resulting prorationing of production gave the industry greater stability and independents a share of the market, although it did not guarantee that they could sell all their oil. With this promise of stability banks became more interested in financing independents. Although most independents relied on local sources of capital, major financial institutions like Continental Illinois Bank and Trust became increasingly involved.

The golden years for independents were those of the decade following the end of World War II. In subsequent years, demand slackened, imported crude oil posed major competition, and engineers and technologists replaced promoters and drillers. Independents turned increasingly to politics to protect their interests. Their success was limited, although they benefited from federal tax policies that encouraged drilling investment by high income professionals who had little knowledge of oil and, in many cases, little appreciation of the risks they incurred. A new domestic oil boom occurred after the oil embargo of 1973 and lasted until the early 1980s. Many entered the independents' ranks during that period, only to be wiped out in the decline that started in 1981.

For readers unfamiliar with oil industry history this is a good introduction to the role of an important and largely unrecognized or romanticized element of it. One will look in vain, however, for detailed accounts of the rise of such famous wildcatters as H. L. Hunt or Hugh Roy Cullen, despite the promise on the dust jacket. The case studies of different strategies by other independents are well chosen but too brief to satisfy. In the end the common denominator for success appears to have been luck.

ARTHUR M. JOHNSON
University of Maine

KRISTINE FREDRIKSSON. *American Rodeo: From Buffalo Bill to Big Business*. College Station: Texas A&M University Press. 1985. Pp. xiii, 255. $18.95.

This book is a 1982 report on "the greatest but least understood American sport" (p. 96). After a century of struggle for respectability and solvency, rodeo has become a well-organized, well-attended, well-publicized, and well-paid—but still controversial—athletic event. Kristine Fredriksson is qualified to do the reporting, having attended rodeo school for six years and having worked as a production assistant. She is now a museum assistant and assistant professor of history at Texas Tech.

The beginnings of rodeo, she says, were, first, informal roping and riding contests ("cowboy fun," [p. 10]) and, second, the Wild West shows that proliferated after 1882. Roping and riding exhibitions became popular in the East, with shows in Boston and New York, and even in Europe; through the two world wars they gained in vitality in spite of major obstacles.

One major difficulty was the popular conception of the cowboy as an "open air delinquent" (p. 13). The image did not change until at least recent times when some of the real range cowboys were replaced by less volatile young men, many of whom were college educated. A second obstacle was the difficulty in establishing strong central control. Organizations came and went—the Rodeo Association of America (1929), the Cowboys' Turtle Association (1936), the National Rodeo Association (1942), the International Rodeo Association (1946), and others. In time, order was achieved, the cowboys enjoyed a better image, and some could make a living on the circuit.

The greatest problem, however, was the opposition of animal protection groups, whose leaders campaigned mightily to abolish the sport. Strong opposition still exists, but the Rodeo Information Commission (1967) and similar bodies have argued

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that "to people in rodeo, the sport has always been a humane activity" (p. 167). Professionals know how to handle stock, and the stock is too valuable to abuse. Nonprofessional rodeos are the real problem. In spite of opposition, rodeo is now firmly established. Over six hundred fifty professional rodeos offered 13 million dollars in prizes in 1982. Some contestants traveled in airplanes, and numerous schools taught the techniques to "cowboys" who never saw a ranch.

Fredriksson's book is well organized, documented, and edited. My criticisms are minor: Fredriksson does become an attorney for the defense in the three chapters on humane-society opposition, she barely mentions female competitors (Joyce Roach's *The Cowgirls* [1977] is not listed or mentioned), and little of the drama of the rodeo spectacle comes through. Her book aims only at assembling the facts about an American sport with a long and interesting history, however, and this it does with conspicuous success.

C. L. SONNICHSEN
Arizona Historical Society
Tucson, Arizona

JOE WILLIAM TROTTER, JR. *Black Milwaukee: The Making of an Industrial Proletariat, 1915–45*. (Blacks in the New World.) Urbana: University of Illinois Press. 1985. Pp. xvii, 302. $24.95.

Begin this book at the end. Buried on page 264, after six statistical appendixes, is an appendix called "Afro-American Urban History: A Critique of the Literature." In it, Joe William Trotter, Jr., sets out the philosophic and historiographic rationale for his history of the black community in Milwaukee.

Although he acknowledges the importance of the "ghetto synthesis" writers from W. E. B. DuBois to Gilbert Osofsky (and those who followed in his wake), Trotter argues that their stress on hostile black-white relations, and the resulting emergence of a physically separate black ghetto, have diverted scholarly attention from the "central concern with questions of economic and class relations" (p. 276). Taking his cue from labor historians like Herbert Gutman and David Montgomery, Trotter says he will write his history of the black working class of Milwaukee from the inside out, stressing the role that blacks themselves played, acting, and not merely reacting, to the dominant white world.

True to his word, Trotter gives us a detailed inside look at the development of black Milwaukee. The assiduousness of his research (it is hard to imagine that Trotter has not used every conceivable source) plus the tiny size of the Milwaukee black community (barely ten thousand as late as 1945) sometimes makes the reader think that Trotter has introduced every Afro-American in Milwaukee, one by one. But what emerges is a strong sense of the internal dynamic of a small group of people struggling to cope with the socioeconomic changes from World War I to World War II.

Despite the frequent use of the term "proletarianization," Trotter does not focus entirely on the black working class. Some of his most interesting material deals with the complex divisions within the black community, not only between the middle class and the working class but also within the middle class itself. Indeed, Trotter's emphasis on the role of blacks in determining their own fate sometimes seems to clash with the realities of white discrimination. But, in the final analysis, Trotter's refusal to oversimplify gives the book a rich, if conflicting, texture.

Because the city was a small backwater in the migration of blacks to the North, the history of black Milwaukee is not intrinsically important. But, as an intensive case study of the interplay of forces within the black community, this book provides us with a fascinating picture of a process in which the making of an industrial working class is actually a step up, not down, from the workers' previous condition.

STEVEN M. GELBER
University of Santa Clara

ROBERT W. OZANNE. *The Labor Movement in Wisconsin: A History*. Madison: State Historical Society of Wisconsin. 1984. Pp. xi, 290. $20.00.

This work comprises two books in one binding. The first 170 pages is a straightforward account of the history of organized labor in the Badger State. The final section, 70 pages in length, is a study of the struggle to organize Wisconsin's paper industry in the period 1900–42. Robert W. Ozanne makes little effort to unite the two parts.

Both "books" rest on extensive archival and other primary materials. Copious quotations from the records of union-busting paper companies provide a good look at the seamy underside of corporate America. Ozanne's account of the grim, but ultimately successful, effort to bring unionism to the state's pulp and paper workers helps document a neglected chapter in the history of twentieth-century unionism.

The main section of the book both rewards and disappoints. Ozanne duly chronicles the unique aspects of Wisconsin's labor history: the impact of Milwaukee socialism, the state's pioneering labor legislation, the sharp conflict between the American Federation of Labor and the Congress of Industrial Organizations (CIO) in the 1930s and 1940s, and the distinctive role of the University of Wisconsin in labor affairs. Ozanne's account of the rivalry be-

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tween socialists and nonsocialists in the State Federation of Labor is balanced. His treatment of the issue of communism in key Wisconsin CIO locals and in the State Industrial Union Council, while dismissive of the legitimacy of left-wing perspectives, is subdued, in contrast to the strident tone found in his influential doctoral dissertation of 1954.

Ozanne is strongest on the important institutional details of Wisconsin labor history. The reader emerges with a clear picture of the structure and functions of Wisconsin unions, and especially of the activities of the State Federation of Labor. There are episodic chronicles of important strikes and organizing campaigns. The legislative battles of the 1910s and 1930s receive more sustained treatment.

Throughout the book the focus is on labor leadership. Except for his vignettes of working-class life in nineteenth-century timber towns and ethnic neighborhoods, Ozanne devotes little attention to the lives of the men and women who composed the rank and file. Indeed, the further the narration advances into the twentieth century, the more abstract becomes the existence of flesh-and-blood workers. Accounts of the rise of the CIO, the impact of New Deal legislation, and the factional conflicts of the 1940s include little discussion of ordinary working people. Other than general comments about the mechanization of factory work, there is virtually no consideration of changing job structures and their implications for workers on and off the job.

Disappointing, too, is Ozanne's avoidance of historiographical controversy, as evidenced, for example, in his Whiggish treatment of labor legislation of the Progressive and New Deal periods. Ozanne's general unwillingness to extend himself intellectually makes the book less challenging and engaging than it might have been in view of the author's long experience in labor education and his extensive research. He invokes Richard Lester's four-stage model of labor-management accommodation as his principal heuristic device, but he shows little interest in exploring its implications or pursuing other interpretive strategies. Informative on a basic institutional level, Ozanne's book ultimately bogs down in complacency and lack of intellectual energy.

ROBERT H. ZIEGER
Wayne State University

JERRY LEMBCKE and WILLIAM M. TATTAM. *One Union in Wood.* New York: International or Harbour, Madeira Park, British Columbia. 1984. Pp. x, 229. $7.95.

This book is a detailed, meticulously researched account of the efforts to build an industrial union in North America's forest product industry. It begins with the "one big union" of the Industrial Workers of the World, continues with the Communist party's Trade Union Unity League and its wood affiliate (the National Lumber Workers' Union), and deals finally and most intensively with the International Woodworkers of America (IWA). The last organization was formed in 1937 when the woodworkers severed their ties to the Sawmill and Timberworkers' Union, an affiliate of the American Federation of Labor's United Brotherhood of Carpenters and Joiners, and, in its place, formed the IWA and affiliated with the Committee of Industrial Organizations, which in 1938 became the Congress of Industrial Organizations (CIO). Within a few years, the IWA was the largest affiliate of the CIO and the Canadian Congress of Labour on the West Coast.

Of other works on the IWA, perhaps the best known is Vernon Jensen's *Lumber and Labor* (1945). But Jensen's study covers only the first four years of the union's history, whereas *One Union in Wood* carries it to the 1970s. Moreover, Jerry Lembcke and William M. Tattam challenge Jensen's thesis that rank-and-file anticommunism within the IWA crystallized in 1941 to oust, by democratic means, the union's communist leadership. Jensen calls this episode "trade union democracy at work."

Lembcke and Tattam argue that the issue was "more complex than the simple rank-and-file anticommunism portrayed by Jensen" (p. vii). For one thing, they demonstrate convincingly that because of organizational skills and programs the communist militants were able to secure the support of many union members. For another, they present considerable evidence to show that decisive interventions against the IWA's communist leaders, such as the forced resignation of Harold Pritchett as international president, were made not by the rank and file but by the U.S. and Canadian governments and the national offices of the CIO and the Canadian Congress of Labour. The means used, moreover, were distinctly not democratic.

In *Communism, Anti-Communism, and the CIO* (1981) Harvey A. Levenstein challenged the widely accepted view that the CIO national office took a hands-off attitude toward communists in its affiliates until the post–World War II years. In their examination of events in the IWA from 1940 to 1942, Lembcke and Tattam confirm Levenstein's thesis. This emerges clearly in the role played by Adolph Germer, assigned by John L. Lewis in 1940 to take over the IWA's organizing program. Germer immediately proceeded to launch a crusade against the left-wing of the union. What was involved, the authors establish, was not whether communists would dominate the union but whether the union would be built along industrial principles that would be "a force for social change within the lumber industry" or along the lines advocated by Germer—

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"the restoration of 'respectable' business unionism akin to that of the AFL" (p. 89).

Based on wide research in archives, oral histories, unpublished and published documents, and all relevant secondary works, this book deserves a place beside Roger Keeran's *The Communist Party and the Auto Workers Union* in adding to our knowledge of the role that communists played in building the American labor movement during the rise of industrial unionism.

PHILIP S. FONER
Lincoln University
Philadelphia, Pennsylvania

THEODORE SALOUTOS. *The American Farmer and the New Deal.* (Henry A. Wallace Series on Agricultural History and Rural Studies.) Ames: Iowa State University Press. 1982. Pp. xviii, 327. $25.75.

In this succinct and well-written monograph the late Theodore Saloutos has provided agricultural historians and New Deal scholars with an excellent overview of the evolution of agricultural policy from the post–World War I era up to 1938. His study traces the origins of farm problems and chronicles the formulation, as well as the implementation, of New Deal farm programs. Although most of the general information in this book does not transcend that found in other studies, Saloutos has synthesized massive amounts of material in a concise and readable form that provides valuable insights into the difficulties involved in carrying out cumbersome and extremely complex programs on a national scale.

Although sympathetic to the New Deal per se, Saloutos is cognizant of the inherent contradictions within New Deal farm policy (such as paying farmers not to produce when millions of Americans were suffering from want), the bitter personality clashes that took place, conflict over ideology, the ferocity of political infighting, and the administrative tensions between staffs of huge bureaucracies. He succeeds in integrating much detailed data pertaining to the genesis of programs for government intervention, the causes of economic dislocations in the rural sector, and the drift of political crosscurrents, all of which influenced proposed solutions for the ills of agriculture.

In Saloutos's account of federal management of agriculture, President Franklin D. Roosevelt appears primarily as a distant figurehead who made use of the ideas and talents of others while serving as the final arbiter in reconciling serious differences of opinion. The Agricultural Adjustment Administration (AAA) dealt with commercial farmers as if they collectively constituted a single industry. Thus, the AAA aided farmers in general, claims the author, but was unable to cope with individual injustices (concerning, for example, black sharecroppers and tenants). Although conceding certain failures, Saloutos nevertheless defends the New Deal against "leftwing critics" (p. 269) by arguing that it did the best it could under adverse circumstances.

Although Henry A. Wallace is portrayed as being important in carrying out New Deal farm programs, the author does not focus sharply on the key role that FDR's secretary of agriculture played either as an outstanding rural leader or as a New Deal philosopher. Wallace emerges from this study as a kind of cardboard character moved by the actions of others rather than as the germinal thinker and bellwether figure he actually was. Curiously, the only biography of Wallace ever cited by Saloutos is the outdated 1947 version by Russell Lord.

Conspicuously absent from the author's interpretive framework is his previous strongly held view that the New Deal farm policy was influenced concretely by significant ideas promulgated by the Populists. Saloutos's more intense research dealing with the economic and social views of the coterie of agricultural economists, farm leaders, and rural editors (who constituted an agrarian establishment that gave birth to the AAA) seemingly convinced him that this elite group did not fit into the Populist mold.

Saloutos's coverage of New Deal farm programs concentrates on the Midwest and South. Additional treatment is given to the reciprocal trade issue and such non-AAA agencies as the Rural Electrification Administration, Resettlement Agency, Farm Security Administration, Federal Emergency Relief Administration, Farm Credit Administration, and the Tennessee Valley Authority. Unfortunately, this book does not deal with FDR's farm legislation during World War II, which had such troublesome repercussions on the postwar period.

In many ways New Deal farm policy did not end in 1938. In a politicized and institutionalized form its costly and cleverly disguised subsidization mechanism is still with us, as are the farm problems it attempted to solve.

EDWARD L. SCHAPSMEIER
Illinois State University

LARRY G. GERBER. *The Limits of Liberalism: Josephus Daniels, Henry Stimson, Bernard Baruch, Donald Richberg, Felix Frankfurter, and the Development of the Modern American Political Economy.* New York: New York University Press. 1984. Pp. viii, 425. $40.00.

In this book, Larry G. Gerber argues that the American "liberal ideological consensus" has inhibited confrontation of the social and economic problems rooted in the processes of capitalism. Focusing

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on the beliefs and careers of Josephus Daniels, Henry Stimson, Bernard Baruch, Donald Richberg, and Felix Frankfurter, from the Progressive era through the New Deal, Gerber finds that all five men adhered to a liberal ideology that assumed the essential beneficence of capitalism and thus precluded a thoroughgoing attack on the problems of inequitable wealth and power distribution. These men acknowledged that reforms of capitalism were needed but believed radical change would threaten the very bases of American freedom.

The author has tried to combine a study of his five individual subjects with a sweeping critique of the liberal reform tradition. Accordingly, much material does not deal directly with the five individuals, nor did the author have to focus on the beliefs and careers of these men to reach his overall conclusions. In this respect the book is primarily valuable not as a close study of the individual subjects but as a broad survey of the liberal reform tradition from a radical ideological perspective. In it the lives of Daniels, Stimson, Baruch, Richberg, and Frankfurter essentially are used to help substantiate the author's overall interpretive themes.

As a critique, the book is often persuasive. But Gerber's concept of "liberalism" is broadly conceived and freighted with notions that, though associated with liberal belief, are not necessarily intrinsic to it. What the author conceives as a critique of liberalism is really a critique of liberalism's connections with capitalism. Thus, Gerber writes not only that "the fundamental tenet of the liberal tradition" was "the freedom and well being of the individual" but also that "the central tenet . . . of the liberal creed was a faith in a . . . private enterprise economy" (pp. 74–75). Granted that American liberals have characteristically linked their liberalism to capitalism, liberalism in its essence may nevertheless be seen as a set of values not inherently linked to capitalism or to any other economic system. The liberals' assumption that individual "freedom and well-being" were necessarily based on a "private enterprise economy," therefore, may be understood not as an inherent flaw of liberalism as such but as a consequence of liberalism's continued association with capitalism.

From this perspective the book is not really about the "limits of liberalism" per se but rather about the limits of a liberalism unable to free itself from an association with the ideological restrictions of an assumed capitalism. The alternative need not be the author's "socialized economy," but a liberalism free to explore and choose from among various economic alternatives those most conducive to the implementation of its own essential values.

THEODORE ROSENOF
Mercy College

RICHARD PFAU. *No Sacrifice Too Great: The Life of Lewis L. Strauss*. Charlottesville: University Press of Virginia. 1984. Pp. x, 314. $17.95.

The literature of recent American history has needed a biography of Lewis L. Strauss, for until now scholars have had to depend on Strauss's self-serving *Men and Decisions* (1962). As the authorized biographer of Strauss, Richard Pfau was granted early access to the large collection of family-held papers. I opened this book with an uneasy feeling that Pfau's interpretation might be biased toward Strauss, but that notion proved unfounded. Pfau has succeeded in penetrating the formidable exterior of Strauss to tell us what moved this key and controversial actor in a significant era of American history. He not only documents Strauss's ambitious nature but also skillfully points out the defects in the man's character. Pfau's critical, objective analysis of the way Strauss's background, training, and personality affected his conduct of public business is an important contribution to historical literature.

Born into a middle-class Jewish family in 1896, Strauss quickly acquired wealth and power through his drive and ability. He secured an appointment as private secretary to Herbert Hoover in 1917, and, using contacts made through "the Chief," this mostly self-educated, articulate young fellow earned financial success through partnership in the investment house of Kuhn, Loeb and Company. Meanwhile, he gained a naval reserve officer commission, a position he used during World War II to advance to flag-officer rank as special assistant to Navy Secretary James V. Forrestal. In 1946, President Harry S. Truman appointed him as an original member of the Atomic Energy Commission (AEC). Before leaving in 1950 to return to Wall Street, he established the detection system that picked up radiation from the Soviet Union's first atomic explosion in 1949, and he strongly advocated the hydrogen bomb program. On the latter issue, he sided against his fellow commissioners as well as the majority of scientists, including J. Robert Oppenheimer, on the AEC's prestigious General Advisory Committee.

In 1953, President Dwight D. Eisenhower appointed Strauss to the AEC as chairman. Before this he had always operated as a subordinate or as part of a team, but during the next five years he had to face public pressure alone for his controversial decisions: the Dixon-Yates contract, the development of commercial nuclear power by private enterprise, advocacy of continued atmospheric atomic testing, and his position against Oppenheimer as a security risk. Pfau fully details these issues, pointing out Strauss's personality and leadership traits that ultimately led to humiliation when the U.S. Senate rejected him for the post of secretary of commerce.

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Above all, Strauss was a patriotic American. The nation was so important to Strauss that on occasion he willingly abused the power entrusted to him if he believed the end justified the means, even going so far as to authorize surreptitious entry and wiretaps. Pfau objectively criticizes this behavior without denigrating the accomplishments of this important American. His balanced assessment allows us to judge Strauss fairly.

GEORGE T. MAZUZAN
U.S. Nuclear Regulatory Commission

ARTHUR SELWYN MILLER. *A "Capacity for Outrage": The Judicial Odyssey of J. Skelly Wright.* Foreword by FRANK M. JOHNSON, JR. (Contributions in American Studies, number 74.) Westport, Conn.: Greenwood. 1984. Pp. xiv, 242. $29.95.

Arthur Selwyn Miller warns us at the outset of this book that he has not written an "orthodox" biography of J. Skelly Wright, who ranks with Learned Hand and Jerome Frank among the most celebrated federal circuit court judges in recent American history. Instead, we will be given "a personalized view" of Wright's judicial career, with special attention to several substantive areas of constitutional adjudication, including race relations, administrative law, freedom of expression, and criminal justice. In each of these areas Wright often displayed, according to one of his former law clerks, Abraham D. Sofaer, a "capacity for outrage" (p. 5).

Unfortunately, it is Miller's outrage that is more frequently on parade in this volume because he uses Wright's career as a useful backdrop for his own tirades against Oliver Wendell Holmes, Felix Frankfurter and his acolytes, white racism, big business, big government, the military-industrial complex, and a host of jurists who support an insensitive, exploitive, and repressive legal order. Even Wright, that paragon of liberal, judicial activism—"Saint Skelly" and "Judas Scalawag" to his enemies—does not escape Miller's wrath on several occasions because of his deference to administrative experts and his reluctance to mount a frontal attack on the national security state.

Miller provides only the most cursory sketch of Wright's career before he ascended the federal bench as a district judge in New Orleans in 1949. While still in private practice, Wright represented Willie Francis, the poor illiterate black youth who was twice subjected to Louisiana's electric chair in one of this century's most hideous examples of capital punishment. The Francis case, Miller argues, was the transforming moment in Wright's public life. From then on, he spurned Frankfurter's brand of judicial restraint and devoted his life to the pursuit of social justice through law. That may be so, but, acting as United States attorney in New Orleans several years after the Francis case, Wright helped purge Robert Himmaugh and other suspected Communists from the National Maritime Union.

Miller offers a useful summary of Wright's leading opinions on issues such as school desegregation, administrative law, the First Amendment, and criminal procedure, but he does not venture an integrative hypothesis that might explain the intellectual foundation of Wright's jurisprudence and the connections among its various branches. And he does not provide comparisons with other circuit court judges who, like Wright, faced the manifold challenges of dispensing justice in cold war America. In fact, Miller believes that little justice has been dispensed by American courts since the 1930s, except in a devious attempt to deflect popular protest and shore up a crumbling social order. No mention is made of Wright's views on civil law, although *Williams* v. *Walker-Thomas Furniture Co.*, perhaps his most famous opinion, is in the realm of contract jurisprudence.

The book is not a stylistic masterpiece. Too many paragraphs contain three or four undeveloped ideas; digression is piled on digression, polemic on cliché. Miller has flogged a few moldering corpses—legal positivism, law as a science, judicial restraint, process jurisprudence—in a manner often reminiscent of the old legal realists, but Wright still awaits his biographer.

MICHAEL E. PARRISH
University of California, San Diego

GILBERT WARE. *William Hastie: Grace under Pressure.* New York: Oxford University Press. 1984. Pp. x, 305. $25.00.

Gilbert Ware has written a useful biography of William Hastie, the first black federal appellate court judge and one of the most significant lawyers and legal educators in the United States. The study is based primarily on oral interviews, Hastie's papers at Howard Law School, and materials in the Howard University Archives and Moorland-Spingarn Research Center. Hastie's career followed in the footsteps of that other incomparable black lawyer, Charles Hamilton Houston, who was his role model, mentor, and colleague. Both graduated from Amherst College, earned doctor of juridical science degrees at Harvard Law School (where they were on law review), taught at Howard Law School, and helped plan and influence the NAACP's grand assault on racial discrimination in the days before *Brown* v. *Board of Education.*

Hastie also served as civilian aide to the secretary of war during World War II, struggling for almost three years to open opportunities for black soldiers

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and to end racial discrimination in the military. He finally resigned in protest, which stimulated further efforts by the armed services toward desegregation. Hastie served in the Virgin Islands as a federal judge from 1937 to 1939 and as governor from 1946 to 1949. His effective campaigning to obtain the black vote for Harry S. Truman in the 1948 election was rewarded by his appointment to the Federal Court of Appeals for the Third Circuit. But opposition to appointing a black to such a high judicial position led to false charges of alleged Communist sympathies, and his confirmation took place only after months of investigation.

Legal landmarks in Hastie's career include winning protection for picketing as an exercise of free expression in the New Negro Alliance case in the 1930s, a Supreme Court victory over the Texas white primary, and ending racial discrimination in teachers' colleges in Virginia. As important as the cases was the group of able black lawyers—including Spottswood Robinson, William Bryant, and Robert Carter—whose education he shepherded at Howard Law School.

Ware cites as one example of Hastie's disciplined "grace under pressure" his refusal to argue that racial segregation in interstate transportation violated the Fourteenth Amendment in *Morgan* v. *Virginia* (1946). Instead, Hastie insisted that the case could be decided on the grounds that state laws regulating interstate transportation were impermissible because the Constitution left the field entirely within the power of Congress. In winning the case, he restricted his argument to the Constitution's commerce clause because he thought the court might otherwise simply affirm the separate but equal doctrine of *Plessy* v. *Ferguson*. The NAACP strategists preferred to build their case bit by bit until the court was prepared to strike down separate but equal in *Brown* v. *Board of Education* some eight years later.

One can quibble about certain aspects of the book. For example, Hastie was not the first black federal judge; Robert Terrell, appointed by Woodrow Wilson to the Municipal Court in the District of Columbia, was the first. Also, one could have wished Ware had followed the model of Genna Rae McNeil's *Groundwork: Charles Hamilton Houston and the Struggle for Civil Rights* (1983) and provided more insight into the personal life, character, and motivations of Hastie. But these are minor matters that do not detract from this work's overall value for students of twentieth-century American history and the law.

MARY FRANCES BERRY
Howard University

D. MICHAEL QUINN. *J. Reuben Clark: The Church Years*. Provo, Utah: Brigham Young University Press. 1983. Pp. xvi, 334.

When J. Reuben Clark, Jr., assumed the position of second counselor to Heber J. Grant, president of the Church of Jesus Christ of Latter-day Saints, in 1933, it was after a distinguished government career that included positions as solicitor general for the State Department, undersecretary of state, and U.S. ambassador to Mexico. Yet at age 61, when he accepted the call to serve his church, he had not achieved his highest aspirations: to be the U.S. senator from Utah, the U.S. secretary of state, or a justice of the U.S. Supreme Court. These goals, he believed, he had to sacrifice out of a deeply felt commitment to his religion. Yet when, at the end of his life, after having served his church as counselor to three presidents for some twenty-five years, he was approached about a biography, he insisted that it should focus on his public career, to the virtual exclusion of his church service.

Although Frank W. Fox carried out Clark's wishes in his authorized *J. Reuben Clark: The Public Years* (1980), D. Michael Quinn's thoroughly researched, judicious, and perceptive study of Clark's church years makes clear that, whatever Clark may have thought, his contribution to the Mormon church was anything but insignificant. Still, for the general reader Clark's contribution to public life is of greater interest than his service to his religion. It is true that during his public years Clark remained virtually aloof from his church. The reverse, however, cannot be said of his church years, which were marked by extensive public service. Even hopes for high office were revived when Alf Landon, in 1936, offered Clark the position of secretary of state in case of a Republican victory.

As an ardent believer in nineteenth-century ideas concerning the U.S. Constitution and as a "free enterprise" ideologue, Clark was an implacable foe of Franklin Roosevelt and the New Deal (although this did not prevent FDR from making use of Clark's expertise in international affairs). A hardline isolationist, Clark resolutely opposed American entry into World War II and harshly condemned use of the atomic bomb against Hiroshima and Nagasaki. He had nothing but contempt for the United Nations. Yet, although he was a staunch anticommunist, he was "unalterably opposed" (p. 100) to NATO and even proposed American accommodation with the Soviet Union to end the cold war. Harshly criticized at the time, he has been vindicated on some of these views, although not on his deeply ingrained racial prejudices, which even sympathetic critics have found difficult to reconcile with his avowed Christian principles.

For the most part, his Mormon followers saw him primarily as a revered church leader who held the line against modernism. Few were aware, however, that one of his most important contributions to his church was the establishment of an administrative

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and organizational framework that moved Mormonism effectively into the twentieth century.

It is to Quinn's credit that, although this is an authorized biography, he has resisted not only external pressure but also self-censorship to cover up aspects of Clark's life that may well run counter to the image modern Mormons would prefer to present to the world.

KLAUS J. HANSEN
Queen's University

ANTHONY HEILBUT. *Exiled in Paradise: German Refugee Artists and Intellectuals in America, from the 1930s to the Present.* Boston: Beacon. 1983. Pp. xiv, 506.

Among the many dislocations caused by Nazi tyranny was the emigration from the Continent—especially, of course, from Germany. It was perhaps most conspicuously a migration of the German mind. By now there have been many investigations of this phenomenon, much of the pioneering work having been done by Laura Fermi, Donald Fleming and Bernard Bailyn, and H. Stuart Hughes. Recently, in the published proceedings of a symposium held by the Smithsonian Institution (*The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930–1945*, [1983]), Jarrell C. Jackman appropriately reminded the reader that the exodus of artists and scholars should be viewed in terms not merely of numbers and large generalizations but also of particular people, their fears and hopes, their courage and accomplishments.

Anthony Heilbut's book explores the path suggested by Jackman: it is a passionate probing into the complexity of the problem of uprootedness and adjustment in a new world. We are made to understand that the United States, traditionally the home for refugees, could and should have admitted more applicants than it actually did; that there was a good deal of resistance to the absorption of immigrants; and that many of the refugees found themselves confronted with a civilization that seemed wholly alien and that they considered superficial, materialistic, and vulgar. Yet the *bei uns* mentality among the exiles, which led to invidious comparisons between life in the new setting and life "back home," was but part of the dialectics of acculturation. What counts, after all, is the achievement, which was impressive.

Besides singling out Theodor Adorno, Bert Brecht, Thomas Mann, and Hannah Arendt for special treatment, Heilbut attempts to present a panorama of exile experiences and contributions. But the view he offers is both too scattered and too narrow. Heilbut does not attempt to impose order, and the reader is overwhelmed with anecdotes and gossip at the expense of systematic analysis. Furthermore, the author tends to be mesmerized by the mainstream of the political and intellectual Left among the exiles. Names like those of the economists Gottfried von Haberler and Friedrich von Hayek and the political philosopher Eric Voegelin do not even appear in the index; they certainly should have had a place in this volume.

The exiles' contribution has been generously summed up by H. Stuart Hughes as the deprovincialization of American scholarship, especially in the social sciences. This summation is on balance correct. But it should not make us forget how much German scholars benefited from their exposure to America, in particular to its openness and pragmatic vitality. It is out of place, then, to emphasize, as does Heilbut, the host country as the land of Congressman Martin Dies, Father Charles Coughlin, and Gerald J. K. Smith. For this was at the same time the country of Eleanor Roosevelt, Alvin Johnson, and William Allan Neilson (whose middle and last names are misspelled in the book), who opened themselves up to the newcomers. The result was a splendid cross-fertilization of various kinds. Although the holistic approach of the Gestalt psychologists and the theoretical bent of the social scientists seem to have been thoroughly Germanic imports, Mies van der Rohe offered American architects the advantages of a modernist style that was international; he had been Americanized before even setting foot in the New World.

Heilbut, like most historians of the cultural exiles, chooses to focus on high culture. This particular road has already been well trodden. Along these lines Lewis A. Coser's study *Refugee Scholars in America: Their Impact and Their Experiences* (1984) is certainly much more serious and authoritative than the book under review. What remains to be done is to investigate the less flashy people, the exile's families, and the younger generation of exiles. In this day and age of social history, a social history of the cultural exile must still be written. Only when it is will it be possible to make a final assessment of the experience and impact of this particular emigration.

KLEMENS VON KLEMPERER
Smith College

DAVID S. WYMAN. *The Abandonment of the Jews: America and the Holocaust, 1941–1945.* New York: Pantheon. 1984. Pp. xv, 444. $19.95.

Forty years ago the Allied armies smashed through the German front lines and began liberating the death camps one by one. The entry of American, British, and Russian troops into Auschwitz, Belsen, Treblinka, Buchenwald, and the other murder factories revealed to the world that events too horrible to be believed were not too horrible to have happened.

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In this landmark study David S. Wyman lays bare America's complicity in the annihilation of European Jewry. The Holocaust was such a profoundly immoral and monstrously criminal act that there could be no innocent bystanders. And, as Wyman makes clear, the United States was anything but an innocent bystander. Its action—or, more correctly, its inaction—doomed untold numbers of Jews to the ovens.

A generation ago the world was divided into two parts: those places where Jews could not live and those where they could not enter. America, Wyman charges, fell into the latter category. A nation of immigrants, a country that has, over the years, admitted millions upon millions of refugees, dissidents, and oppressed peoples of all kinds, America in the 1930s and 1940s could find no room in her bosom for more than a handful of the tormented Jews of Europe. Indeed, one man, Raul Wallenberg, rescued more Jews than were admitted in the United States during the entire Second World War.

Wyman's story is a tale of shame. It spares no one. The Franklin Delano Roosevelt so long revered by the Jews is unfrocked as a man so totally indifferent to the destruction of European Jewry that he vetoed almost every attempt to save them. He did not wish to denounce the Nazi atrocities nor discuss the fate of European Jews with representatives of a beleaguered American Jewish community. As Wyman concludes, "the era's most prominent symbol of humanitarianism turned away from one of history's most compelling moral challenges" (p. 313).

Perhaps as culpable as the president in Wyman's eyes were the Departments of State and of War, both of which were riddled with anti-Semites. Neither showed the slightest interest in the Jewish tragedy. Indeed, both were deeply involved in scuttling what few attempts were made to rescue some of Europe's trapped Jews. For as long as they could, they kept the lid on the news of the death camps and Nazi extermination policies. They rejected Romania's offer to free seventy thousand Jews, dissuaded Latin American countries from opening their doors to Jewish refugees, tightened immigration procedures so that scarcely any Jew could get into America, sabotaged dozens of plans to move Jews out of Nazi-occupied zones, and refused to bomb the death camps or even railways leading to them. At the same time, as Wyman points out, they were prepared to help non-Jews trapped by the Nazis. Thus, while Jews were being transported to Auschwitz, over one hundred thousand non-Jewish Poles, Slavs, and Greeks were being moved by American troops to safe areas in North Africa.

Wyman's indictment is as unremitting as it is persuading. The media is excoriated for ignoring—or underplaying—stories about the massacre of Jews. The Congress is condemned for its negativism and nativism. And even the Jewish community comes under attack for its disorganization and division. Ultimately, it is, of course, the American people who must share the blame. As Wyman makes clear, the public mood in America was reflected by the behavior of its leaders.

And this is of course the tragedy. What was on trial in America in those years was democracy itself. Although the Nazis planned and executed the Holocaust, it was made possible by the failure of democracies—chiefly, the United States of America—to respond. Following a trail already blazed by Arthur Morse, Henry Feingold, Yehudah Bauer, and, most recently, Monty Penkower, Wyman brilliantly analyzes the reasons for that failure and eloquently describes what might have been done—and why it was not.

Wyman's is a shocking, numbing story and he tells it well. Objective and dispassionate, the book is a model of historical writing, but no one can read it without being moved and angered. There are few heroes in Wyman's tale because there were few Americans who were prepared to help save the victims of the Holocaust. It was their misfortune not only to be foreigners but also to be Jews. Thus, their abandonment was total.

IRVING ABELLA
York University

STEPHEN J. WHITFIELD. *A Critical American: The Politics of Dwight Macdonald.* Hamden, Conn.: Archon. 1984. Pp. x, 179. $19.50.

RICHARD H. PELLS. *The Liberal Mind in a Conservative Age: American Intellectuals in the 1940s and 1950s.* New York: Harper and Row or Fitzhenry and Whiteside, Toronto. 1985. Pp. xi, 468. $18.95.

The aspect of American intellectual history during the 1940s and 1950s that remains the most "current" in the consciousness of historians and of educated Americans generally is the politically engaged work of the academic and literary men and women who wrote for *Partisan Review*, *Politics*, *Commentary*, *Dissent*, *The Nation*, and *The New Republic*. The accessability of this particular swath of our recent intellectual history is being maintained by the steady stream of often-disputatious memoirs now being produced by Irving Howe, Sidney Hook, Alfred Kazin, and other principals. Scholars, too, are now writing extensively about the political history of American intellectuals during World War II and the early cold war. The scholars tend to reinforce the importance of exactly those personalities, problems, episodes, and texts that dominate the memoir literature. This is very much the case with both Richard

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H. Pells's *The Liberal Mind in a Conservative Age* and Stephen J. Whitfield's *A Critical American*.

Neither book has much to offer readers already familiar with the history and lore of the American left-of-center intelligentsia during the period. Whitfield provides a sprightly, sympathetic narrative of the career of an impetuous and independent moralist who, more than any other critic of the mid-century decades, resembled Randolph Bourne and H. L. Mencken. Pells reviews the cultural cold war, liberal "pluralism," the debates over "mass society," the consensus historiography of Richard Hofstadter and Louis Hartz, and the reactions of intellectuals to Senator Joseph McCarthy. Pells consolidates the received view of these episodes well enough to serve as a starting point for students and for others new to the material.

Whitfield's appreciation of Macdonald scarcely succeeds in its "attempt to rebut" the "charge" of Macdonald's detractors that the editor of *Politics* was a fickle "thinker-on-the-run," ready to trade a formulation for a good wisecrack (p. 2). This is a needlessly defensive frame in which to place Macdonald, and Whitfield himself proves too sensitive to the philosophically half-baked, *ad hoc* quality of Macdonald's writings to take seriously this announced mission. Whitfield does demonstrate a certain coherence in Macdonald's political witness, but this book reminds us that Macdonald managed to become a genuinely provocative, occasionally perspicacious, and extraordinarily well-remembered critic exactly while "thinking on the run." A more probing inquiry into Macdonald might have tried to uncover the cultural and psychological roots of this truly distinctive presence in the community of American intellectuals of his generation. This might require more use than Whitfield made of the Macdonald Papers at Yale, "stretching," as Whitfield wearily observes, "across seventy-three feet, packed into 170 archival boxes." These materials became available to Whitfield only after "the basic research for this book was completed" (pp. ix–x). No doubt, the other scholars now working in these materials will have more to tell us, but in the meantime Whitfield's book puts the essentials of Macdonald's career efficiently before us.

Pells's strongest contribution is his opening chapter, "The Intellectuals at War," in which he details the wartime *New Republic*'s "dependence on authority figures of all sorts both at home and abroad" (p. 13). The remaining chapters lack the sharpness of this interesting beginning and tread onward in a tired, "ground-covering" mode. This is a typical sentence: "Weary of the Marxist slogans and radical activism of the 1930's, disappointed by the American labor movement's abandonment of its Depression-style sense of mission, bored by the absence of dramatic social controversies in the 1950's, and increasingly mistrustful of all political movements that incited mass emotions (especially if they resulted in right-wing crusades like McCarthyism), intellectuals sounded ever more skeptical about grandiose projects to remodel economic institutions or human nature" (p. 119). In at least one puzzling respect, Pells does not confirm the received view: if his surprisingly tangential account of Reinhold Niebuhr were all one had to go on, one might surmise that Niebuhr has little more than a shadow to Arthur M. Schlesinger, jr.

Pells's perspective is "critical" in that he is eager to assess the wisdom, courage, and independence of the figures under study. He passes out little stars and demerits to his subjects, judging whether a given argument made by Macdonald or William Barrett or Seymour Martin Lipset is "plausible" or "persuasive." Too often, Pells reveals that he has not decided whether these judgments are his own or those of the most sophisticated of the mid-century intellectuals themselves or those of a timeless, ideal observer: "It was hard to suppress the thought," he informs us, "that [Paul] Goodman's preoccupations with 'manly' virtues said more about his own sexual prejudices than about the current and future predicaments of women or men" (p. 214). For whom *was* it hard to suppress this thought? Or is the thought itself simply Pells's own complaint of the 1980s dressed up as something else? Such failings render his work more moralistic and banal than his *Radical Visions and American Dreams* (1973), to which *The Liberal Mind in a Conservative Age* is an obvious sequel.

DAVID A. HOLLINGER
University of Michigan

MARK MCCOLLOCH. *White-Collar Workers in Transition: The Boom Years, 1940–1970*. (Contributions in Labor History, number 15.) Westport, Conn.: Greenwood. 1983. Pp. xii, 193. $29.95.

In this new study Mark McColloch addresses the lack of scholarship on specific white-collar occupations and the long-term historical development of white-collar work. He selects three groups of white-collar workers—those in banks, electrical manufacturing, and public welfare—and traces their development between 1940 and 1970, focusing on "the evolution of work processes, changes in the composition of the work force and shifting patterns of collective activity by the worker" (p. 5). McColloch does not, however, discuss worker participation in politics.

Despite McColloch's attention to the differences among the occupational groups he studies, he discounts those differences and the differences between white-collar and blue-collar work. He stresses an

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overall trend toward "proletarianization" of white-collar workers and concludes that between 1940 and 1970 there was "substantial movement" of white-collar workers toward "identity" with blue-collar workers (p. 179). That emphasis results from his findings that white-collar workers "found their jobs more routine, less autonomous, and, usually, less skilled" (p. 175); that they experienced decreasing upward occupational mobility, especially in the electrical manufacturing and banking industries; that women workers increased rapidly in banking and public welfare and became concentrated in low-status, low-paying jobs; and that real wages increased at roughly the average for manual workers. The intensity of union activity varied in the three industries but followed a common pattern: a period of expansion from 1940 to 1947, a reversal of gains between 1947 and 1951, and a revival during the 1960s, led by public welfare workers.

The book is richest in its coverage of collective bargaining during the period 1940–51. By 1945 nearly half of the white-collar electrical workers were organized, and during the immediate postwar period the pace of unionization accelerated among all salaried workers in the three industries. McColloch concludes that the strongest gains were among groups linked by CIO unions to production workers. He details the attack on the left-wing organizations of salaried workers; that assault, relying on the Taft-Hartley Act, had eliminated significant unionization everywhere but the electrical manufacturing industry by 1951.

McColloch's structural analysis in this brief book is weakened by the lack of systematic quantitative data to define the pattern of relationships among the increasingly complex categories of white-collar work, mobility within and between industries, the sexual and racial composition of white-collar employment, the distribution of wages, and the trends in unionization. Nonetheless, McColloch has lent historical sensitivity to the sociological discussion of white-collar work, especially by identifying the 1940s as a period critical for understanding the organization of white-collar workers. And he has pointed to the value of a variety of important sources in union archives and union newspapers. Thus, he has contributed significantly to the ongoing research that may eventually produce historical scholarship that rivals the impact of C. Wright Mills's *White Collar: The American Middle Class* (1951).

W. ELLIOT BROWNLEE
University of California, Santa Barbara

CURTIS SELTZER. *Fire in the Hole: Miners and Managers in the American Coal Industry*. Lexington: University Press of Kentucky. 1985. Pp. xii, 276.

This book by Curtis Seltzer is the most recent attempt to explain the evolution of labor-management relations in the coal industry in the post–World War II period. These were the years when the United Mine Workers (UMW), first under John L. Lewis and then under Tony Boyle, worked together with the Bituminous Coal Operators Association (BCOA) to solve the industry's chronic crises of cutthroat competition and overproduction. The result was a joint agreement to maintain labor peace by binding the operators to a fixed wage scale while permitting them to lay off hundreds of thousands of miners. Lewis was able to impose this system on the rank and file because he controlled the union with an iron fist. District autonomy was eliminated and the BCOA and UMW "slid into conspiracy" (p. 79). During this post-1945 period Lewis used UMW organizing drives to help the BCOA eliminate competition, and the union became increasingly corrupt. This set the stage for the Boyle regime and the rebellion that brought Arnold Miller to power.

These developments have been analyzed and reanalyzed by journalists and social scientists for much of the past decade and a half. This book, which relies completely on secondary works and lacks historical perspective, adds little to the understanding of the period. Seltzer does not seem to be aware that as early as the 1880s miners and operators attempted to solve the problems caused by cutthroat competition and overproduction by negotiating a contract that would have bound them to a fixed wage scale. By 1898 an industrywide joint contract was firmly in place and continued to operate to maintain labor peace for twenty-five years. In contrast to Seltzer's bleak description of this period, real wages steadily increased and working conditions improved substantially under the joint conference system. This equilibrium was shattered during the 1920s when nonunion West Virginia coal came to dominate the market and northern operators were forced to break their union contracts. Lewis presided over this dismal period, but in the 1930s he took advantage of the protection provided by the New Deal to reconstruct the UMW, which he used as a base to build the Congress of Industrial Organizations.

Seltzer never comes to grips with the heroic role that Lewis played during the depression decade, and he thus cannot understand why as late as 1970 thousands of miners still had photographs of Lewis on their living room walls. They tolerated his corruption because they remembered the Lewis of the 1930s and the benefits that the joint conference had brought to their fathers and grandfathers.

For all his supposed sympathy for the miners, Seltzer never manages to enter their world. His description of the pre–World War II period is far too brief. Futhermore, he makes no attempt to

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incorporate the insights of the new labor historians into his work or to understand the miners' cultural milieu and its value systems. David Corbin's recent book is cited, but its significance is not appreciated. In recent years coal miners have been the focus of a number of oral history projects that have attempted to explore the experiences of the 1910s, 1920s, and 1930s in imaginative ways. Unfortunately, Seltzer does not appear to be aware of this work.

Seltzer's primary focus is on the UMW's post–World War II leadership. The result is a one-dimensional book that sees the miners as passive victims. The author condemns the Lewis-Boyle regime without understanding why the miners supported it for so long.

MICHAEL NASH
Hagley Museum and Library
Greenville, Delaware

LAWRENCE S. KAPLAN. *The United States and NATO: The Formative Years*. Lexington: University Press of Kentucky. 1984. Pp. xi, 276. Cloth $30.00, paper $12.00.

This splendid book, which has taken shape over three decades, demonstrates how much can be accomplished by a good scholar who has mastered a big subject. Lawrence S. Kaplan knows a wide range of diplomatic history as well as the sources of his special topic. Showing that historians have paid little attention to NATO's formative years, he defines and fills a large gap in the scholarship on the Truman period and challenges an assumption that NATO is "of limited importance, at best a by-product of the Truman Doctrine and a cog in the containment plans of the American foreign policy establishment" (p. 204). Although restrictions remain, the growing availability of contemporary sources, especially American materials, facilitated his challenge.

The book combines powerful essays at the beginning and the end and less exciting but careful analyses in between. My favorite, "The Treaties of Paris and Washington: Two Entangling Alliances," is a fascinating essay in comparative history and reveals a historian at play. The author is at his best in defining the interaction among ideas and intellectual positions, as when he deals with the several ideas about the nation's role in the world embraced by Americans after World War II, and in seeking a satisfactory generalization about the significance of the Korean War. The middle chapters move more slowly as Kaplan tries to keep his eyes on all the actors, but his sense and demonstration of the complexity of events and his multiarchival research are obvious strengths.

For Kaplan, the interaction among elites—offical and nonofficial, domestic and foreign—determines the shape of American foreign policy. They include senators, State Department officers, military leaders, journalists, and diplomats in other countries. The role of the president, at least during the Truman period, seems small. Truman takes on great importance in these pages only in the decision to intervene in Korea and in his support for Dean Acheson. Business leaders figure even less prominently.

To Kaplan, it appears, revisionism has come and gone without leaving a large mark. Focusing on individual actors, mainly political ones, he does not emphasize capitalism and capitalists. He rejects a revisionist argument about a causal link between NSC-68 and intervention in Korea. He does recognize economic benefits for the United States from its large role in Europe, but he doubts that the nation was ever as powerful, exploitative, or imperialistic as some have claimed.

In spite of the great difficulty in doing so, the author reaches a conclusion about the performance of the Truman administration. This is difficult because of our inability to know enough about the Soviet Union's intentions at the time. His conclusion is cautiously positive. He doubts that "any other approach would have yielded greater security or prosperity to the NATO allies" or that we should blame Truman for such subsequent events as "Johnson's errors in Vietnam" (p. 174).

RICHARD S. KIRKENDALL
Iowa State University

STEVEN L. REARDEN. *History of the Office of the Secretary of Defense*. Volume 1, *The Formative Years, 1947–1950*. Washington, D.C.: Government Printing Office. 1984. Pp. xi, 667.

After 1945 most federal agencies experienced tension and confusion as they adjusted to a new era following the long years of depression and war. This was especially true of the military establishment. Military appropriations from 1947 to 1950 were on average ten times greater than those of 1939. Yet, as Steven L. Rearden shows, multiplying worldwide commitments, new global conceptions of national security, and novel technologies such as atomic weapons confronted military planners with all the problems of adjustment that other government officials faced.

Secretaries of Defense James Forrestal and Louis Johnson guided the department during these formative years. In 1947 Congress enacted the National Security Act, creating the National Military Establishment crowned by the Office of Secretary of Defense. Although Congress had largely shaped the act according to Forrestal's recommendations, the

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secretary of defense soon found the law badly flawed because it made him coordinator, with little real power, of the three largely autonomous Departments of Army, Navy, and Air Force. In 1949, Congress, responding to the urging of Forrestal and others, established the Department of Defense and gave the secretary authority to direct its activities, not merely coordinate them.

Translating law into practice exacted high personal costs from both secretaries. By 1949 Forrestal was a broken man; in 1950 Truman fired Johnson and in part made him a scapegoat when the Korean War revealed deficiencies in White House and congressional military policy. Even though the United States remained the most powerful nation on earth, the secretaries had to cope with the frustrating fact that political leaders accorded the domestic economy highest priority, holding armed forces budgets down while assuming global commitments requiring military backing.

Changing military roles and missions heightened tension among services already made jealous and suspicious by the long unification controversy. The air force, for example, had emerged from World War II as the glamorous new service, often overshadowing the army and navy. Its power and prestige grew when budget restrictions pushed planners into increasing reliance on long-range strategic bombing supported by an atomic arsenal, which promised more power for less money. The navy felt especially threatened and fought intensely to increase its role in military planning. Such struggles paralyzed the whole military establishment and prevented the Joint Chiefs of Staff from playing the integrative role that Forrestal had envisioned. Still, Rearden believes that gains outweighed failures: the nation assumed new global responsibilities, met a series of international crises (such as the Berlin airlift), and moved to correct some of the weaknesses in the 1947 act.

Unlike many authors of official histories, Rearden avoided writing a dull chronicle of his agency's glories and achievements. He burrowed into a mountain of primary and secondary material and produced a book that clarifies what often seems incomprehensible to outsiders. One disturbing element is that, although the government declassified the information contained in this book, some of the footnoted documents remain classified. Despite this drawback, Rearden's book sets a high standard for succeeding volumes in the series.

WILLIAM E. PEMBERTON
University of Wisconsin, La Crosse

ANDREW J. DUNAR. *The Truman Scandals and the Politics of Morality*. Columbia: University of Missouri Press. 1984. Pp. viii, 213. $24.00.

At best, this book should have been one or two articles. Andrew J. Dunar's thin monograph focuses on the more dramatic scandals of the Truman presidency—those involving White House aides Harry Vaughan and Donald Dawson, the so-called five percenters, and the Reconstruction Finance Corporation and the Bureau of Internal Revenue. To expand this limited inquiry into a book, Dunar explores President Truman's general appointive decisions and earlier relationship with Kansas City "Boss" Tom Pendergast. This more comprehensive approach fails, and the resultant study is a cursory, often deficient account of the Truman presidency.

The strength of Dunar's monograph is the detailed narrative on the so-called Truman scandals. Based on research in the secondary literature and recently opened primary sources at the Truman Library, this book neither adds to the conventional wisdom about these scandals nor offers insight into Truman's administrative and political decisions and the related issue of how congressional involvement in uncovering and publicizing these scandals affected relations between the executive and legislative branches. Dunar's principal contribution is to further our understanding of Truman's personality, administrative style, and attitudes toward the press. Although sympathetic and generally commendatory, Dunar treats these scandals comprehensively and dispassionately, providing support for a more critical appraisal of Truman's political and administrative abilities.

The major deficiencies of this monograph stem from Dunar's failure to place the scandals in a broader perspective by linking them to Truman's major administrative and appointive decisions. Inexplicably, Dunar does not discuss Truman's handling of the Harry Dexter White appointment (1946), which was controversial because of questions about White's loyalty, not about his corruptness. Dunar also does not examine how this case helps in understanding Truman's administrative decisions and political problems—a serious omission, for, as in the case of the corruption scandals, Republicans and the conservative media exploited the White appointment politically. Dunar, moreover, treats only summarily the complex and more troublesome issue of the Kansas City vote fraud case (1946) and ignores the questions raised by a Senate subcommiittee in 1948 about the administration's efforts to limit committee access to relevant FBI documents.

More troubling, given the parallel between the Nixon and Truman presidencies, the author does not review how the Truman White House dealt in 1945 with the question of how to ensure the loyalty of governmental employees to the newly elevated president. Concerned about the influence and loyalty of Roosevelt holdovers on the White House staff and in federal departments, the administration re-

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quested that the FBI wiretap both Washington lobbyist Thomas Corcoran and Treasury Department aide Edward Prichard as well as investigate other aides suspected of leaking information to the press, notably, to syndicated columnist Drew Pearson. These examples suggest that Truman's conception both of governance and of the politics and competence of those staff members on whom he relied for counsel determined how he responded both to the issues of loyalty and corruption.

As a result of his limited research and conceptualization, Dunar's book neither revises nor supports the dominant interpretations of the Truman presidency. Truman specialists, nonetheless, may find some value in his detailed analysis of the scandals.

ATHAN THEOHARIS
Marquette University

EDWARD L. SCHAPSMEIER and FREDERICK H. SCHAPSMEIER. *Dirksen of Illinois: Senatorial Statesman.* Urbana: University of Illinois Press. 1985. Pp. xvi, 269. $19.95.

Who among observers of politics in the United States during the 1950s and 1960s can forget the inimitable Everett McKinley Dirksen? He was perhaps the country's premier political orator of the post–World War II generation. As *Time* once observed, he was wont to utter words that emerged in a soft, sepulchral baritone, undulated in measured phrases, expired in breathless wisps, and chased each other about in dreamy images of steamboat gothic. And it was he who conducted a determined if quixotic campaign to have the humble marigold enshrined as the national flower.

Dirksen, of course, was more than a political showman; he was also a dedicated and tireless public servant. A conservative and a Republican, he doubtless reached the pinnacle of his career in 1964–65 when, as minority leader in the United States Senate, he put his influence in the balance in behalf of the Democratic administration's initiatives aimed at advancing the cause of civil equality. Indeed, without his support neither the Civil Rights Act of 1964 nor the Voting Rights Act of 1965 would have achieved passage through Congress, for only he had the wherewithal to assemble the requisite votes to terminate filibusters by southern senators who sought to forestall the measures by exhausting the patience of their proponents. Unfortunately, Dirksen may have been one of those figures of large historical importance—George W. Norris and Robert A. Taft were others—whom later generations are apt to lose sight of, in the main because writers and teachers of history tend to concentrate on the enterprise of the executive branch of the national government.

Relying extensively on the 1.2 million pages of personal papers and office files in the Dirksen Congressional Leadership Research Center in Pekin, Illinois, Edward L. Schapsmeier and Frederick H. Schapsmeier have composed an admirable biography, one that might be described as a summary biography inasmuch as Dirksen's long life and career could easily have yielded a much heftier volume. They have traced Dirksen's life and career from his early years in a predominantly German neighborhood in Pekin through his sometimes harrowing experience in a balloon company on the Western Front in 1918 and his years as a baker in Pekin to his more than thirty-five years of service in Congress, first in the House and then in the Senate. They have assayed his support of some important New Deal initiatives and his opposition to others during the early years of the presidency of Franklin D. Roosevelt, his uncompromising demands for isolationism in American foreign relations through the summer of 1941 and subsequent repudiation of that policy, his role as the chief spokesman on Capitol Hill for the administration of President Dwight D. Eisenhower, and his exceptionally warm and generally supportive relationship with President Lyndon B. Johnson.

If not totally uncritical of Dirksen—they deplore, for example, his affinity for Senator Joseph R. McCarthy during the latter's search for communists in government in the early 1950s—the Schapmeiers view Dirksen with obvious affection and admiration. They view him as a man who was animated by an abiding love of country, faith in Christian principles, and commitment to the standards and values of the small towns and countryside of middle America. They view him as a man who could transcend ideology and partisanship when the national interest seemed to require, as when he lent unstinting support to the Nuclear Test-Ban Treaty of 1963 and the aforementioned civil rights legislation. In their perspective, then, he was unquestionably a senatorial statesman.

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RHODA LOIS BLUMBERG. *Civil Rights: The 1960s Freedom Struggle.* (Social Movements Past and Present.) Boston: Twayne. 1984. Pp. xxiii, 209. Cloth $18.95, paper $7.95.

ROBERT FREDRICK BURK. *The Eisenhower Administration and Black Civil Rights.* (Twentieth Century America Series.) Knoxville: University of Tennessee Press. 1984. Pp. xi, 287. $24.95.

The Reagan administration may have put civil rights on the back burner, but many historians and social

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scientists continue to treat the subject as the major domestic issue since World War II. As the titles suggest, these books are but two of the latest efforts to set the "American dilemma" in historical perspective, the better to come to grips with the painful combination of progress and retrogression, peaceful change and racist violence that has marked the entire postwar era.

Robert Fredrick Burk is a historian whereas Rhoda Lois Blumberg is a sociologist, yet their books reveal quite a few similarities as well as differences. They both deal, for example, with the gray-flannel conservative consensus of the 1950s (Burk more than Blumberg) and with public policy in the field of race relations. Both focus almost wholly on post–World War II developments, and both happily avoid the homiletic *ipse dixit*s that deface too many works on civil rights.

One of the major differences between the books is that Burk, in effect, makes blacks the object of his analysis because he is chiefly concerned with the Eisenhower administration's racial policies, whereas Blumberg makes them the subject of her study by examining the black freedom struggle, and particularly the actions of the charismatic black figures who led the protean battle.

Although neither work contains any startlingly new insights into race relations or brings to light any newly discovered evidence that would change conventional interpretations, both make a worthwhile contribution to a scholarly field that since 1945 has probably generated more speculation, political action, and writing than any other. Each author has a clear if not brilliant writing style, and the books are exceedingly well edited. In addition, both books contain an adequate index and helpful notes, although only Burk puts the notes at the bottom of the page. Both books also feature an excellent selection of photographs, including one in Burk's book of Eisenhower's only White House meeting with black civil rights leaders on June 23, 1958 (frontispiece). Finally, both authors obviously are steeped in the relevant literature, from which they have extracted numerous apt quotations.

Of the two works, Burk's is unquestionably superior in every way. It is more tightly organized, the writing flows more fluidly and is more detailed, and, most important, it includes a splendid bibliographical essay, such as is unfortunately missing from Blumberg's book. To state that Burk's study is the most comprehensive analysis of Eisenhower's racial policies ever published is indeed no exaggeration, and it thus marks a worthy addition to the scholarly literature both on the civil rights movement and on the Eisenhower presidency. However unfair the old stereotype of Eisenhower as a kindly but bumbling president, Burk deflates Eisenhower revisionism by confirming the conventional wisdom that Eisenhower was truly a reluctant dragon in the field of civil rights and thereby contributed to the anger that black militants expressed in the 1960s. The chief value of Blumberg's book is that it provides a handy overview of the triumphs, tragedies, and personalities of the black freedom crusade. Hers is not the first such summary, but the subject needs to be recapitulated periodically because of its intrinsic importance.

Both books, of course, have their deficiencies. The arguments could have been strengthened by tabular data and more profound analyses of black and white ideologies. Neither author seems to have made much use of interviews, although Burk did effectively mine the materials in the Eisenhower Library, and especially the papers of E. Frederic Morrow, black White House assistant in the 1950s. Both err on the side of timidity rather than enthusiasm and slip all too easily into a narrative mode, substituting cursorily summarized sequences of events for analyses of how and why they came about. Finally, both books will probably be faulted by theory-oriented scholars for being overly cautious, too simplistic, and lacking in theoretical concerns. Each work certainly could have used a more sophisticated concluding chapter. Burk's book, it is true, ends with a skimpy epilogue, but Blumberg's book simply fades away and ends with two quite pedestrian sentences: "It remains to be seen how soon the civil rights coalition can build again. Undoubtedly the struggle will resurface, but when and in what form?" (p. 166). She does go on to add a brief appendix, "Civil Rights as a Social Movement," that represents rather primitive theorizing.

Even though both books go over well-trod ground and fail to move beyond the full and accurate reporting of facts to a theoretical interpretation, they should be welcomed by students of American politics and by general readers, if not by specialists. If neither Burk nor Blumberg boldly seeks to reinterpret the American heritage as such scholars as Louis Hartz, Bernard Bailyn, and J. G. A. Pocock brilliantly did, both authors set for themselves an important, if modest, objective and generally succeeded in realizing it. It is not for us, the readers, to insist that they should have had more lofty, original goals or written different books.

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HERBERT S. PARMET. *JFK: The Presidency of John F. Kennedy*. New York: Dial. 1983. Pp. viii, 407. $19.95.

DAVID BURNER and THOMAS R. WEST. *The Torch Is Passed: The Kennedy Brothers and American Liberalism*. New York: Atheneum or McClelland and Stewart, Toronto. 1984. Pp. 307. $16.95.

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Fascination with the Kennedy family has generated an abundance—and some would say an overabundance—of biographies and television docudramas. Fortunately, Herbert S. Parmet's *JFK* is one of the best of the former. The book, which completes Parmet's two-volume study (*Jack* was published in 1980) of the only Kennedy to reach the White House, is a readable and objective description of the slain chief executive's administration.

In *The Torch Is Passed* David Burner and Thomas R. West stake out a more ambitious task: assessing the public careers of three Kennedys—John, Robert, and Ted—in the context of American liberalism. Judging by the number of recent books with the word "liberalism" in their titles (for example, *The Limits of Liberalism* by Larry G. Gerber [1983], *Liberalism and Its Challengers* by Alonzo Hamby [1985], and *The Unraveling of America* by Allen Matusow [1984]), liberalism bodes well to become almost as popular a topic as the Kennedys. Burner and West readily acknowledge that the word "liberalism" has many meanings, but they define it as "the politics of the welfare state combined with a sense of mission abroad and a sponsorship of civil rights and liberties at home" (p. 5).

Both books being reviewed are solid scholarly works, competently researched and engagingly written, but Parmet's is the more valuable of the two. Although he reveals nothing especially novel about the Kennedy presidency, Parmet nevertheless provides readers with a balanced narrative that does not gloss over Kennedy's (or the Kennedy family's) mistakes and blemishes. Parmet examines the president's less-than-idyllic marriage and his extramarital sexual affairs candidly but with taste, describes Robert's abrasive behavior toward unfortunate subordinates whose answers or actions displeased him, and reduces the qualities of Rose Kennedy from the angelic to the more human. But the author also recognizes JFK's many fine qualities and importance as a leader able to mobilize the spirit and will of the American people and to capture the admiration of millions around the globe.

Parmet's interpretation does not differ from the view of most Kennedy scholars that the young president was a keenly intelligent and attractive leader who had great promise but who delivered relatively little. The author also agrees with the textbook description of JFK as a "cold warrior"—like most Americans at the time—but one willing and eager to work for reasonable accommodation. Parmet is more original with his concept of Kennedy as a survivor who endured pain and overcame countless barriers, such as the natural inhibitions of his personality, to succeed. Parmet subtitled the first volume of his Kennedy biography *The Struggles of John F. Kennedy*, and he continues to emphasize this theme in the sequel. Forced to prove himself repeatedly to his domineering father, JFK knew the world was not a moral place and saw life, which he knew was not fair, as a constant test. Not surprisingly, Parmet identifies JFK as a moderate conservative and a rational idealist.

Burner and West make no attempt to claim that JFK was an impassioned champion of the leftward movement in America. But they argue that Kennedy and his brothers gradually left their conservative past behind and progressed toward more liberal political and social positions. The authors also stress that much of what happened during JFK's years in the White House provided an important impetus for the growth of liberalism. They cite, for example, the beginning of detente, the placement of the federal government clearly on the side of the civil rights movement, the establishment of the Peace Corps, the beginning of a long cycle of prosperity that would foster the social movements of the 1960s, and the initiation of federal involvement in environmental issues.

Robert and Ted Kennedy are actually secondary figures in *The Torch Is Passed*, although the subtitle suggests otherwise. Almost two-thirds of the book focuses on John, and most of that deals with his presidency. One chapter is devoted to Robert and his "leftward journey," and another explores the "heir," Edward. "Epilogue: A Note on Liberal Ideology" concludes the book. The epilogue is shrewdly reasoned, interesting, and informative, but I am not sure that it fits comfortably with the rest of the book. Burner and West offer some thoughtful analysis about American history during the 1960s and provide a succinct summary of Kennedy's presidency spiced with frequent perceptive observations and insights. But trying to trace "the fortunes of modern liberalism against the experience of the political family most closely identified with it," as the book's dust jacket proclaims, at times seems rather forced. Terms such as "technocratic liberals," a "liberal's general," or "educated liberalism" are sprinkled throughout, but their meaning is not always clear. I suspect that Parmet comes closer to describing John Kennedy's real opinion of liberals—or at least of many liberals: speculating about his postpresidential career, JFK told one especially dear woman friend that he questioned retiring to the life of a college professor, because he already had his fill of pedantic intellectuals who "used the word *liberal* so much that it made him gag" (p. 307).

In sum, although Burner and West have written an interesting and worthwhile book, I do not see it standing the test of time as well as Parmet's work. If not quite the definitive work on Kennedy, *JFK*, in tandem with *Jack*, will serve nicely until something better appears.

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D. DOUGLAS DALGLEISH and LARRY SCHWEIKART. *Trident.* (Science and International Affairs.) Carbondale: Southern Illinois University Press. 1984. Pp. xxii, 502.

This excellent book should be required reading for officers of all the armed services as well as legislators, journalists, and those scholars who investigate the problems of national defense in the nuclear era. It should be read because D. Douglas Dalgleish and Larry Schweikert have managed to put the Trident missile and the radically advanced submarine developed as its launching platform into an even-handed historical perspective, shedding a great deal of light on the bureaucratic, legislative, and defense contracting processes that characterize the acquisition of weapons systems in general.

A central thesis of this volume is that most previous accounts of the Trident have been simplistic, distorted, or downright erroneous, failing to put the subject into its full context, not only in relation to other weapons such as the B-1 bomber and silo-launched intercontinental missiles but also in relation to the plans and practices of other NATO nations. What makes this study remarkable is that the authors did not have privileged access to sources at the Departments of Defense and Navy. Apart from a number of interviews, they relied almost entirely on published sources, thereby demonstrating that, if one is willing to dig for the facts, it is possible for the public to secure the information needed for rational decision making in the defense arena. In a democracy the selection of weapons is ultimately a political problem. Unfortunately, the debate over Trident, as over other weapon systems, has too often been characterized by strident headlines about "cost overruns" and "excessive profits" or TV pundits deploring unconscionable delays and inefficiency with scant regard for the facts of the case.

To be sure, mistakes were made by all the participants. Congress contributed by irregular funding; the navy was at fault in forcing the contractor to negotiate a fixed price contract when only 10 percent of the design details had been settled; the contractor made unduly optimistic delivery schedules; and the authors are quite blunt in attributing a great many of the difficulties to the acerbic personality of Admiral H. G. Rickover, whose dominance over design stultified the creative capabilities of both contractor and navy engineers.

Against the record of bureaucratic mistakes and personality clashes the authors build an impressive case to show that the Trident project has in fact been a resounding success; it provides the nation with a deterrent capability (the most advanced Trident missile has a range of more than six thousand miles) with far less vulnerability than bombers and silos. Moreover, it offers a wide range of attractive options for cooperation with our NATO allies without the drawback of bases on foreign soil.

Among the other strengths of this volume, two stand out. It is massively documented, and serious students will find the endnotes a treasure trove of sources. Moreover, the book is well written; even the most technical details are readily comprehensible to the layman. A skimpy index and the failure to annotate the bibliography are weaknesses.

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CANADA

HUGH A. DEMPSEY. *Big Bear: The End of Freedom.* Lincoln: University of Nebraska Press or Douglas and McIntyre, Vancouver. 1984. Pp. 227.

Big Bear's story is not unique. A squatty, rather homely man, he rose to prominence in 1870 as a political leader of the Plains Cree of Canada. The decline of the buffalo, the building of the Canadian Pacific Railroad, and the growing attempt of the Canadian government in Ottawa to tame the Plains, however, tested Big Bear's political skills. He sought to maintain unity among the many Cree and Chippewa bands then inhabiting the main tributaries of the Saskatchewan River Valley, and he tried to preserve his Plains culture and the buffalo herds that made it possible. But he failed on both accounts. The destruction of the buffalo, the introduction of Christianity, and the growing settlement of the Canadian frontier forced Big Bear to sign a treaty in 1882 relinquishing claims to his Plains homeland and accepting a reservation.

The story might have ended here had the Canadian government been more judicious in its handling of reservations. Throughout the mid-1880s, conservative politicians cut back on expenditures, causing starvation on reservations and encouraging unrest. When the mixed-blood Louis Riel attempted to take advantage of this discontent in 1886 and launched a frontier rebellion, Big Bear's people joined the outbreak, killing nine men at their agency near Frog Lake on April 2. Although Big Bear tried to prevent the killing and never did participate in the fighting, he had been such a stubborn advocate of Indian rights that, once the rebellion collapsed, he was captured and sent to prison. His strength gave out while being held at Stony Mountain Penitentiary, near Winnipeg, and he died shortly after being released in 1888.

Those scholars looking for a unique cultural, or ethnohistorical, assessment of Canadian Plains Indians will not find it in this book. Hugh A. Dempsey is

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an excellent storyteller and the book is well written, but the reader will learn little about the complex interethnic relations of Canadian Plains Indians or, for that matter, about the motives and goals of Canadian Indian policy. Several generalizations also lack support. We learn, for example, that Big Bear was a keenly intelligent man, but Dempsey presents little convincing evidence to demonstrate the chief's intellectual abilities. Indians who seemingly adopted Christianity and accepted reservations are dismissed as weak-minded pawns of government officials. Dempsey also states that Cree social structure suddenly "collapsed" under the strain of whiskey, starvation, and thievery, while Big Bear and his people were visiting Montana in 1879–80 (p. 93). But this obviously important change is not discussed in any detail.

Despite the weaknesses, this book will be read and enjoyed. Dempsey should certainly be credited with attempting the biography of a man for whom few sources exist. More and better books of this sort will be a welcome contribution to the expanding biographical literature on the Indians of North America.

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LATIN AMERICA

LESLIE BETHELL, editor. *The Cambridge History of Latin America.* Volumes 1 and 2, *Colonial Latin America.* New York: Cambridge University Press. 1984. Pp. xx, 645; xx, 912. $140.00 the set.

Since the volumes to be reviewed here are the first two of *The Cambridge History of Latin America* to appear, it seems appropriate to say something about the purpose, scope, and organization of the work as a whole. As revealed in the general preface, its declared aim "is to produce a high-level synthesis of existing knowledge which will provide historians of Latin America with a solid basis for future research, which students of Latin America will find useful and which will be of interest to historians of other areas of the world" (vol. 1, p. xiv). For the purposes of the series, Latin America is defined as the predominantly Spanish- and Portuguese-speaking regions of the Americas south of the United States and, "by convention," Haiti. The old Spanish borderlands north of Mexico and the former Northern European colonies in the Caribbean and around its circumference are for the most part excluded.

The emphases of the series will be heavily weighted toward modern Latin American history. Of the projected eight volumes to be published in chronological order, two cover the colonial period (roughly the sixteenth through the eighteenth centuries); one will treat the independence and postindependence periods (ca. 1790 to 1870–80); and the remaining five will deal with the years 1870–80 to the present, of which three volumes will concentrate on the decades since 1930. Although "the decisive impact of . . . external forces" is acknowledged, primary attention will be given to the evolution of "internal structures." Brazil will "receive the attention it deserves" (vol. 1, p. xv).

The two colonial volumes reviewed here are the collaborative work of thirty-two chapter authors and the editor. Volume 1 has three parts. The first, "America on the Eve of the Conquest," deals with pre-Columbian Indian history. Part 2, "Europe and America," moves from three chapters on conquest and early settlement to five on the development of imperial systems. The subject of part 3 is "The Church in America." Volume 2 has four parts: part 1, "Population," contains two chapters; parts 2 and 3 deal mainly with internal economic and social structures; and the subject of part 4 is "Intellectual and Cultural Life."

A number of good things can be said about these volumes. The best is that for the most part the editor was successful in recruiting well-known and highly regarded contributors, among the most eminent being J. H. Elliot, Charles Gibson, Fédéric Mauro, and John Murra. Contributors, moreover, appear to have taken their assignments seriously, and most wrote original scholarly essays. Some of these deal with subjects not treated explicitly in previous general histories of Latin America, or they synthesize more or less familiar subjects in new ways. In the first category, Asunción Lavrin's "Women in Spanish Colonial Society" deserves mention; in the second, I was particularly struck by Murdo Macleod's "Aspects of the Internal Economy of Colonial Spanish America" and Stuart Schwartz's "Colonial Brazil, c. 1580–c. 1750: Plantations and Peripheries." A second merit of the series is that Brazil, as promised, gets the "attention it deserves"—about 30 percent of the text, well distributed among the several major parts. A third is the valuable up-to-date bibliographical essays supplementing each chapter.

The series, however, has some visible structural shortcomings. Although perhaps not part of my charge, I feel obliged to criticize the "presentist" weight of the entire work. The general preface justifies the allocation of five of the projected eight volumes to the last hundred years of Latin American history on the grounds that the modern period has been neglected by professional historians and therefore presumably needs to be illuminated. This is all very well for persons who know their way around in the Latin American field, but those who do not will get a distorted view of the region's past.

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I have some more specific criticisms of the first two volumes. One is what has been left out or downplayed. Only about a dozen pages are devoted to the "European Background." According to the preface to volumes 1 and 2, the reason for this cursory treatment is that pre-Columbian European expansion and the European discovery of America "belong more properly to the history of Europe" (vol. 1, p. xviii) and that in any case there is already a vast literature on these subjects. It can be argued equally logically that pre-Columbian Indian history, which consumes 146 pages, is not really "Latin" American and is more properly the domain of anthropologists. In any case, much as already been written on the subject. I also find it strange that, unlike economic and social structures, governmental structures are not treated explicitly in volume 2.

A second criticism is that the parts of the two volumes lack linkage, progression, and cohesion. Most of the material in volume 1 dealing with imperial systems and virtually all of that in volume 2 analyzing internal structures range through all three colonial centuries. But these two aspects of colonial history cannot be separated neatly, and the attempt to do so results in a considerable overlap between the two volumes, in subject matter if not treatment. "The Church in America" seems simply to be appended at the end of volume 1 as if it was neither a part of imperial systems nor an internal structure.

A last criticism is the editorial decision to eschew any explicitly comparative treatment of Brazil and Spanish America because, as stated in the preface to volumes 1 and 2, "they have, on the whole, two different histories, . . . [and] there are . . . few historians competent and willing to write comparatively about [them]" (vol. 1, p. xix). I think they have as many common features as different ones, and there are historians who can compare (for example, Stuart Schwartz). In any event, if such comparisons cannot be made, doubts arise about the validity of packaging both histories in the same container.

Having made these criticisms, it is only fair to temper them by saying that the features criticized are inherent in a collaborative work of this magnitude. What can be included depends heavily on the expertise available. And even the sternest or most politically astute editor cannot impose a rigorous schema on a score or more of widely dispersed scholars from several disciplines who are accustomed to doing things their own way.

In sum, prospective readers who want a wide-ranging collection of modern, "high-level" essays on colonial Latin American history will welcome this work warmly. The needs of those who seek a well-ordered, well-balanced history will not be fulfilled.

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LUIS WECKMANN. *La herencia medieval de México.* In two volumes. Mexico City: El Colegio de México, for the Centro de Estudios Históricos. 1983. Pp. x, 394; x, 397–837.

The historiography of sixteenth- and seventeenth-century Mexico has often interpreted the colony from a Renaissance–Counter Reformation perspective, a milieu conditioned by the political and ideological currents of Habsburg Spain. Christian humanism of the Spanish Renaissance was quickly overwhelmed by Counter Reformation defensive postures. A contrary historiography claims that the prevailing politics, economy, and social organization were deeply rooted in the Middle Ages.

This work by Luis Weckmann, Mexico's most famous medievalist, develops these ideas in depth. Introduced by the eminent Belgian medieval scholar Charles Verlinden and by the dean of Mexican colonial historians Silvio Zavala, these two well-researched volumes totaling 837 pages are a milestone of colonial historiography. Weckmann traces what Verlinden calls the "continuing of the Middle Ages in the behaviour and mentality of the conquerors, warriors, friars and priests" of New Spain, and he praises Weckmann for eschewing impressionistic writing in favor of "solid and documented historical method" (p. 2).

In reviewing such a monumental work, one can only give an impression of its contents and interpretations. Weckmann spent more than thirty years preparing these volumes, which are firmly grounded in Spanish medieval history, law, art, and literature. The author's own legal training pervades the chapters on empire, political structure, and transfer to Mexico of medieval institutions such as the encomienda. Medieval geographical concepts and legends of amazons, giants, pygmies, and monsters pervade the initial sections. Influences of the French school of *mentalités* can be seen in the explications of the conquerors' views of themselves as epic heroes and as New World men of the age of chivalry.

Ten fascinating chapters on the church in New Spain from the conquest until mid-seventeenth century trace supernatural aspects of the conquest—St. James, the Virgin Mary, and Lucifer—as well as medieval precedents for problems of missiology, evangelization, and religious syncretism as Iberian Catholicism and Mesoamerican religions collided. Popular devotion as well as Spanish mysticism are explored within the context of medieval transfer. Miracles, miraculous images, doctrine, rites, and liturgy are traced from medieval origins. Seven chapters deal with political structure and the political economy of Habsburg Mexico, casting the discussion within concepts of Dante's "world empire," territorial sovereignty, entailed estates, tribute sys-

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tems, craft guilds, confraternities—even including the evolution of weights and measures. The urban nature of Spanish colonization in Mexico is stressed in many of the final thirteen chapters, laced with discussions of colonial culture, *colegios*, universities, printing, historiography, popular literature, and much more.

At the end of his perceptive introduction, Zavala points to the massive amount of research that went into these volumes and finally introduces one caveat—the temptation to emphasize medieval roots to the detriment of other influences on Habsburg Mexico, particularly Renaissance and Baroque. The present reviewer has, of course, often maintained the Renaissance influences in early Mexico were also exaggerated in Mexican colonial historiography and that the Mexican Counter Reformation and Baroque were more neomedieval than anything else. Whatever roots splendidly trained medievalists may see in Mexican history from the conquest to 1650, Weckmann has established the major questions for research and discussion. The magnificent bibliography (pp. 737–815) is proof of that.

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ROBERT W. JOHANNSEN. *To the Halls of the Montezumas: The Mexican War in the American Imagination*. New York: Oxford University Press. 1985. Pp. xi, 363. $25.00.

The Mexican War did more than add territory to the United States. It raised American presige abroad a notch or two, it expanded presidential powers, and, as Robert W. Johannsen shows, it also gave the American citizen, already proud of his accomplishments, a greatly enhanced view of his potentialities. Whether, as Johannsen says, it "carried the United States into the modern world" (pp. 12–13) by showing America's ability to wage a complicated offensive campaign may be open to question. An amazing succession of victories was superimposed on the cresting Romantic movement in popular literature, and the combination was spread over the whole country by the newly improved penny press. All these forces created a fantasy world of patriotism and heroic deeds, especially during the first half of the war, that refurbished American history back to the revolution and renewed Americans' devotion to their old ideals, now a little tarnished.

Johannsen's account of the impact of the war on the American imagination is based on thorough examination of the enormous fugitive, nonpolitical literature of the 1840s. (He omits congressional speeches and campaign polemics.) He is at his best in dealing with solid facts—for example, his chapter on the behavior of American troops in Mexico should be read to supplement any military history of the war. When he deals with less substantial topics (such as patriotic fervor, hero worship, chivalry, attitudes toward Mexico, and war poetry), he is apt to subordinate penetrating analysis to summaries and descriptions of everything from cogent commentary to long-winded vaporings on glory. In any history of movements of thought a certain amount of this is necessary. Still, before the end of the book Johannsen seems not only optimistic but also indulgent, strongly implying that the war was justified and generally good for both country and people.

This is nowhere more apparent than in the chapter that concentrates most of what he has to say about one of the most intense antiwar movements in our history. Johanssen thinks antiwar propaganda had little effect on the general public, and his account of this literature is pallid and chilly in contrast with the warm, full-bodied, almost loving details about supportive literature in the rest of the book. Antislavery opposition to the war does not appear at all until page 175; we learn only incidentallly that William H. Prescott, so widely read for his heroic history of Cortez, thought the war "mad and unprincipled" (p. 245); Ralph Waldo Emerson's damning epitaph for the war as "a dose of arsenic" is passed over in a mild paraphrase. Although Johannsen has given us a nearly definitive statement of the prowar case, his readers are urged to turn to John H. Schroeder's *Mr. Polk's War: American Opposition and Dissent, 1846–1848* (1973) for a comparable view of the other side.

And yet at the end of the book, after still another quotation about the glorious future, Johannsen concludes, "The Mexican War *did* bring America to a crossroads—but it was a crossroads of a different kind, unanticipated, baffling, and fraught with peril for the republic" (p. 312). All the more baffling to the reader, one might add, since the author has not prepared him for it.

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JAMES C. CAREY. *The Mexican Revolution in Yucatán, 1915–1924*. (Westview Replica Editions.) Boulder, Colo.: Westview. 1984. Pp. xx, 251. $22.50.

In the last several years a number of books and articles on the regional dimensions of the Mexican Revolution have appeared. An area that has attracted major interest, particularly through the work of Gilbert Joseph and Allen Wells, is the Yucatán peninsula. That Yucatán would attract so much attention seems strange at first glance because

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it was peripheral to the major struggles of the revolution. Indeed, as Joseph points out in his own fine book, the revolution took place outside the peninsula, and its effects were scarcely felt until General Salvador Alvarado was sent by the central government of Venustiano Carranza to be the governor of the state of Yucatán.

The changes that took place in Yucatán in the postrevolutionary period, however, are of interest for a number of reasons. First of all, it was an area of considerable social stratification, with major divisions between large landowners and those who worked the land. Many of these workers were Maya Indians. In economic terms, the region was dominated by the large henequen plantations, and the production of henequen was closely associated with the U.S. market and particularly with the International Harvester Company, which bought enormous amounts of the fiber for binder twine. Yucatán had two remarkable revolutionaries, Alvarado and Felipe Carrillo Puerto, who served as governors of the state in the immediate postrevolutionary period and who made major strides in imposing social and economic reforms and in mobilizing the workers and the peasantry. Unfortunately, these changes disappeared almost immediately after Carrillo Puerto's death in 1923 during the De la Huerta rebellion, which was strongly supported by Yucatán's hacendados, great opponents of his Socialist party. Ironically, Alvarado also participated in the rebellion, but he was fighting for the other side.

James C. Carey's book is a welcome addition to this growing literature. Carey's work is largely narrative in nature, supplementing Joseph's and Wells's analyses of Yucatán's social and economic structures. Further, Carey focuses more closely on political events, some of which are highly dramatic. He also contributes additional insight into the composition and maneuverings of political factions, personalities, and parties in the state. Such information is of great value and further points up the difficulties of attributing political splits in postrevolutionary Mexico to ideology or even to class interests, a problem illustrated by the participation of Alvarado and Carrillo Puerto on opposite sides in the De la Huerta rebellion.

Although Carey's book suffers from a lack of careful editing—a not uncommon problem with facsimile editions of transcript manuscripts—his book is interesting and the information valuable. It is yet another significant contribution to the regional history of the Mexican Revolution.

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JANE M. RAUSCH. *The Tropical Plains Frontier: The Llanos of Colombia, 1531–1831*. Albuquerque: University of New Mexico Press. 1984. Pp. xv, 317. $29.95.

The llanos of Colombia are vast tropical plains that extend east of the Andean Cordillera Central. They have an inhospitable climate with seasonal extremes of drought March through December and rain of 60 to 170 inches January through November. Occupying 20 percent of Colombia's territory but housing less than 2 percent of its 27 million inhabitants, the region has long had a marginal impact on Colombia's history, except during the wars of independence. From the El Dorado mirages of sixteenth-century conquistadors to the illusionary dreams of progress of twentieth-century planners, the failure to understand this hostile world has led many to grief. This is the main theme of José Eustasio Rivera's *La vorágine* (1924), where the idyllic vision of an urban highland Colombian is shattered by the harsher realities of an uncivilized frontier.

Jane M. Rausch is the first to give historical form, substance, and meaning to this region. Her book is thus a major accomplishment and a significant addition to Colombian historiography. She writes with imagination and verve, which makes for stimulating reading. She traces the history of the llanos and their interactions with the main centers of population in the Colombian highlands from 1531 to 1831. She follows the searches for fabled El Dorado and the reduction of Indians to forced labor and mission life. Of special interest is how the highlands extended their rule over the llanos by establishing towns, encomiendas, missions, and ranches. Even the expulsion of the Jesuits in 1767 did not, as is commonly thought, stop this growth. Other religious orders expanded the mission system. Most of this, however, came crashing down in the wars of independence, as the conflicting claims of settlers, missionaries, and Indians were swept away by the barbaric realities of war.

Few mistakes appear, but the routes of conquistadors Jorge de Espira and Alfonso de Herrera are reversed on map 3 (p. 25); the correct year for Indian tributaries and encomenderos in columns 2 and 3 of table 1 (p. 42) is 1603 rather than 1653; and, if the towns of San Juan and Santiago disappeared from maps by 1810 (pp. 144–47), they still appeared annually in tithe records from 1810 to 1833.

In a larger context Rausch suggests the Colombian plains had more in common with the history of the lowlands of eastern Bolivia, Peru, and Ecuador, whereas the llanos of Venezuela more closely paralleled the Argentine pampas. Finally, Rausch underscores the dramatically different impact the llanos had on the historical experiences of Venezuela and Colombia. Caudillism tended to reign supreme in Venezuela. The plains provided easier access to the

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highlands and were demographically and economically far more important than their Colombian counterpart. The Colombian elite, however, used the more imposing Andean Cordillera Central to keep llanero caudillism out of the highlands. In the process the Colombian political system was more civilian and traditional in tone and character.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

BILL ALBERT and ADRIAN GRAVES, editors. *Crisis and Change in the International Sugar Economy, 1860–1914*. Norwich, England: ISC. 1984. Pp. xii, 381. $16.80.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of my book *Greek-Soviet Relations, 1917–1941* (*AHR*, 89 [1984]: 478–79), S. Victor Papacosma fails to mention the book's sources (Greek, German, French, Italian, American, British, and especially Soviet), describes inaccurately its contents, and misses the most important events; e.g., he writes: "the Soviets showed a lack of interest in Greece, even after the Italian invasion in late October 1940."

Actually, before and during the Italo-Greek war, Moscow *inter alia* negotiated with Rome for an Italo-Soviet pact, and, upon Greece's occupation by Germany, severed relations with Greece to appease Hitler—events not mentioned in the review.

The reviewer is apparently also unable to make clear to the reader which views in the review are his and which are the author's; e.g., he writes: the Greek Communist Party "members were early victims of John Metaxas's harsh rightist dictatorship." This is his view, but it is not clarified.

The severance of relations by the USSR, a major event in the book, is not noted by the reviewer; Metaxas's treatment of the communists, a domestic issue, is. The bold stand of Metaxas versus the Axis, hailed in the West and acknowledged by the Soviet press—quoted in my book—is not mentioned by the reviewer.

The review adds that my last chapter "provides in tedious, often repetitive detail the various interpretations surrounding the significance of the Greek stand against the Germans and the consequent delay in the Axis invasion of the Soviet Union."

This statement is doubly inaccurate: (a) the last chapter also includes, *and was prompted by*, the views of those who do not accept the thesis that the Greek war in 1941 delayed the attack on the USSR; (b) the reader is also given the erroneous impression by the reviewer that the last chapter is a mere rehash of old views. In fact, since 1945, for over 35 years, German generals and leading European historians often alleged that the spring came late in Eastern Europe in 1941 and that a late thaw and river floods (not the German campaigns in the Balkans) delayed the attack on the USSR. In response, my book (last chapter) is the *first study ever* to contain detailed meteorological reports on Eastern Europe (including meteorological tables especially compiled for this writer by the West German and Polish weather services) refuting the above allegations.

The recently published volume III of the series *Das Deutsche Reich und der Zweite Weltkrieg* (The German Reich and the Second World War) of the Militaergeschichtliches Forschungsamt, the official West German military history research institute, states (p. 483) that my last chapter offers a "complete discussion" on the topic; also, my study is one of only three on this subject mentioned in the said volume. Yet the reviewer calls my last chapter "tedious." The reviewer adds: "Regrettably, the author has ineffectually presented his important subject. The reader is bombarded with countless undigested quotations, long explanatory footnotes, and one-sentence paragraphs."

Has the reviewer consulted, e.g., Max Beloff's *The Foreign Policy of Soviet Russia, 1929–1941*, Mario Toscano's *Designs in Diplomacy*, Louis Fischer's *Russia's Road from Peace to War*, Churchill's *The Second World War*? Quotes in my book are also for the benefit of scholars not familiar with the Russian language.

The reviewer continues: "A judicious editorial hand would have cut the book's length in half and increased its interpretive analysis." But if the research for the book was extensive, as he admits, what should one do—discard half of it or present it in capsule form? For his own book on a Greek

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military coup of 75 years ago, *The Military in Greek Politics: the 1909 coup d'état*, the reviewer used 254 pages; but for a book on Greece and the USSR covering 25 years, including war periods and Hitler's campaigns in 1941, the reviewer feels some 300 pages would suffice for a heavily researched and documented account!

(Space does not permit me to respond to other criticisms.)

The latest edition of the *Directory of American Scholars* (1982) indicates that the reviewer has published the book on the 1909 coup and ten articles which, according to a list of their titles he supplied to the *AHR*, deal with Greek topics; he has not published anything specifically on the USSR or World War II (German-Soviet war/Balkan campaigns). Thus, he cannot be seriously considered a specialist in either. A proper assessment of my book would require someone fully acquainted with Soviet sources (and fluent in Russian) and very familiar with the bibliography of World War II relating to Germany and the USSR.

ANDREW L. ZAPANTIS
New York

PROFESSOR PAPACOSMA REPLIES:

For a number of subjects Andrew Zapantis's book is a gold mine of detail. Unfortunately, this achievement has been accomplished at the sacrifice of cohesive analysis. The prime example of this deficiency is the total absence of any discussion of the general foreign policy trends of Greece and the Soviet Union for the interwar years.

I would argue, moreover, that thorough research for a book need not necessarily bear a direct relation to the number of printed pages in the final product. Critical and interpretive analysis, organization and synthesis, in conjunction with extensive research, are intrinsic to the important mission of the academic historian. By contrast, Zapantis's writing style and approach seem to reflect more his undergraduate training and career in journalism. His book, an expanded version of a doctoral dissertation in history submitted to a European university, contains countless block quotations, one-sentence paragraphs, three- and four-page chapters, and numerous explanatory footnotes often consuming more space on the page than actual text. On the basis of prevailing norms associated with the training of history graduate students in American universities, I question whether in its present form his publication would have been accepted for the Ph.D. in this country. In turn, I trust that I have not read into Zapantis's references to Messrs. Beloff, Toscano, Fischer, and Churchill an attempt to equate his work with theirs.

S. VICTOR PAPACOSMA
Kent State University

TO THE EDITOR:

The October 1984 issue carried a review of my book *The Lust for Power: Nationalism, Slovakia, and the Communists, 1918–1948*, written by Professor Stanislav Kirschbaum (*AHR*, 89 [1984]: 1113–14). Mr. Kirschbaum called my book "a confusing study." I am afraid, however, that he tops in confusion my book.

In his review he stated as follows: "A group in Slovakia was aware that the Slovak Republic, in other words, Slovak statehood, responded to the national aspirations of the Slovak people and best ensured their development. This was made abundantly clear in the document prepared for the Soviet leadership on conditions in Slovakia in the summer of 1944 by Gustáv Husák. It is astounding that Jelinek makes absolutely no mention of this document, which, when it was finally published in its entirety in the West in 1979 also showed Husák and others around him *preferred the incorporation of Slovakia as a republic in the USSR to a return to Czechoslovakia. When this was not possible they pushed for the federalization of Czechoslovakia.* (The emphasis is mine.)

In my book, on page 73, I wrote: "Šmidke presented his Moscow hosts with a document prepared by the Party at home. The authors would like to see Slovakia as an autonomous republic in the Soviet Union. If this solution was impossible, then the best solution would be a federated Czechoslovakia in which Slovakia would enjoy self rule."

I wish somebody would explain to me the ("astounding") difference between the two statements. Further, the said document has been well known to Slovak historians at least since 1965. (See *Slovenské Národné Povstanie roku 1944*, Bratislava 1965, pp. 214–15.) It was printed in 1969 under the name "Čo bolo pred Povstaním. Neznámi dokument z Povstania" [What happened before the Uprising: An Unknown Document from the Uprising] (*Nové Slovo*, nos. 33 and 34, August 8 and 21). Friends sent me the first part of this document, and it is listed in the bibliography of my book on page 158. Evidently Mr. Kirschbaum is not familiar with the original printing of the article. Finally, the document was not first published in the West in 1979. The first Western printing was in 1969. (See "Ueber die Slowakei vor dem Volksaufstand im Jahre 1944" [*Wissenschaftlicher Dienst für Ostmitteleuropa*, Marburg, 11 November 1969, pp. 717–37].) One should study Slovak history, historiography, and documentation before

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criticizing the works of others. Such lack of scholarship as displayed in the said review can easily confuse everybody.

Furthermore, I believe that academic decency and high ethical standards should have convinced Professor Kirschbaum to abstain from reviewing a book in which his father is treated critically. Evidently he would not miss the opportunity to settle the account with Jelinek.

YESHAYAHU JELINEK
Ben-Gurion University of the Negev

PROFESSOR KIRSCHBAUM REPLIES:

Thank you for giving me the opportunity to reply to Professor Jelinek's letter.

The document in question is probably one of the most important in the history of the Communist movement in Slovakia, for it reflects very clearly the dilemmas the leadership of the Communist Party of Slovakia faced especially in 1944 and which I outlined in my review. Jelinek not only gives it short shrift but fails to identify its author. Even in his bibliography, he does not give the complete reference and identifies it by its subtitle "Neznámi dokument z Povstania" rather than by the full title which he gives in his letter. That is the reason why I did not realize that the passage from his book which he quotes in his letter referred to it. Still, this does not take anything away from my main point, which is that Jelinek shows a serious lack of judgment on the importance of the document.

Its importance, in addition to content, can also be gauged by the vagaries of its publication. In the first place, when Communist scholars in Slovakia were allowed in the 1960s to rewrite the history of the 1944 events, this document was omitted from the mammoth collection of documents edited by Vilém Prečan, *Slovenské Národné povstanie: Dokumenty* (Bratislava, 1965). When it was published in 1969 in *Nové slovo*, it was edited and its authorship was not revealed. As Karol Šmidke was the one who took it to Moscow, many scholars assumed that it was his memorandum. In 1978 the West German publication *Der Spiegel* (XXXII, no. 15, 10 April 1978) published those passages that had been edited in 1969 and established that Gustáv Husák had been its author. In 1979 the Czech émigré review *Svědectví* (no. 58, 1979, pp. 361–82) published it for the first time in its *entirety* in Slovak with an accompanying explanation by Vilém Prečan, who had obtained a copy of the original and ascertained its authorship.

I used the 1969 version in my doctoral dissertation, *Le stalinisme et la Tchécoslovaquie* (Paris, 1970), and I also refer to it in my article "Federalism in Slovak Communist Politics," *Canadian Slavonic Papers*, XIX (4), 1977, p. 446, note 13. However, in a subsequent article "Slovak Nationalism in Socialist Czechoslovakia," *Canadian Slavonic Papers*, XXII (2), 1980, pp. 226, note 32, I correct my earlier reference to Šmidke and point to *Der Spiegel* and *Svědectví* as the latest sources. If anyone is guilty of a lack of scholarship, it is certainly not I. All these texts are available in most libraries, yet they do not appear in Jelinek's bibliography.

Serious scholars accept not only a plurality of views, but also fair criticism. It was Jelinek's lack of judgment on this document, among other things, that made me consider his book confusing. That is why I did not give it a favorable review.

S. J. KIRSCHBAUM
Glendon College
York University

TO THE EDITOR:

Reading the review of Professor Walter B. Simon's *Österreich, 1918–1938* (*AHR*, 90 [1985]: 450), I am amazed about the standards applied by Professor Kitchen to a book of real value. I want to emphasize that Professor Simon and I were in prewar years on different sides of the barricades and that I do not wholly approve of his views. Yet his volume is a shining example of fairness and objectivity. It is a major and almost unique effort in seeing that tragic period of our history dispassionately, putting blame and recognition on both sides: the guilt for the bloody events has been quite evenly distributed.

Yet the German language is not an easy one: the Christian Socialists never called themselves Socialists but "Socials": they would have rejected the Socialist label outright. *The Treaty of Versailles has nothing to do with the breakup of the Dual Monarchy*. The Constitution of 1929 is exactly the same as the one now in vigor and it had not been tampered with by politicians of the right. The Constitution of 1934, however, had nothing at all in common with the Constitution of 1929 (or of 1919). Hitler never sent a republic packing, because Austria after 1934 ceased to be a republic. Austrians after 1933 were not in two, but in three major camps. Professor Simon never called the Communists of Austria a "bunch of disoriented and deracinated Jews," but spoke about such Jews as articulating Communist views. (Among the rank and file of Austrian Communists the Jews were hardly a majority.) This wrong citation is made in such a way as to make Professor Simon appear to be an "anti-Semite"—our author *is* a Jew and proud to be one!

Professor Simon's book is of greatest value to Austrian readers and fulfills a real mission. It should

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have been reviewed by somebody cognizant of the simplest facts of our history.

ERIK VON KUEHNELT-LEDDIHN
Lans, Austria

PROFESSOR KITCHEN REPLIES:

Professor von Kuehnelt-Leddihn's emotional defense of a man who once stood on the other side of the barricades, and with whose views he is still not entirely in agreement, is a moving example of the forgiving tolerance for which Professor Simon pleads in the book in question. Unfortunately it does rather miss the point.

Few would dispute that the German language has its difficulties, but the labeling of Seipel's party as "Christian Socialists" was due to a printer's error and not to mistranslation. This might have been assumed since "socialists" appears in lower case. "Christian Social" does sound distinctly odd in English, and whoever made this change should not be too severely castigated. The use of the "Treaty of Versailles" as a cover term for all the peace treaties is terribly sloppy, but it is useful and widely used shorthand. No doubt I should have said "the Treaties of St. Germain and Neuilly," or perhaps we should learn to use such delightful German phrases as "the Parisian Suburban Treaties." I would not dispute the fact that the constitutions of 1919 and 1934 were widely different, nor that there were constitutional changes made in 1929. My point was that the right set out to undermine the constitution of 1919, and did so very successfully. One may bemoan this or applaud it, depending on one's political views, but one cannot deny it. I must confess that I was surprised to learn that Austria ceased to be a republic in 1934, and on this point alone plead guilty to the incognizance of which I stand accused. That Austrians either sympathized in one way or another with Italian Fascism, or were violently opposed to it, is Professor Simon's view. As sweeping generalizations go, this is not a bad one, and I am at a loss to know why Professor von Kuehnelt-Leddihn is so upset about it. Perhaps he feels that the fence-sitters should be given their due place in the history books as upholders of true democratic virtues. Whether the writer is Jew or gentile I find talk of "disoriented and deracinated Jews" as forming the nucleus of the Austrian Communist Party singularly unattractive. Maybe it is because this is the kind of language much favored by those whom Professor Simon would no doubt abhor, or perhaps because such phrases provoke an emotional response but are empty of real meaning.

If this modest volume "fulfills a real mission" and is unique in its dispassionate fairness and objectivity, then Austria today must be in dire straits. I like to think that this is not so, and that there are enough Austrians who, instead of wallowing in the exculpatory sentimentality of the "Austrian Tragedy," know that guilt, like wealth, talent, and intelligence, is not "evenly distributed" and that human beings, in spite of all restraints and difficulties, forge their own destinies.

MARTIN KITCHEN
Simon Fraser University

TO THE EDITOR:

May I intrude briefly on your space to comment on your review of Ms. Joan Peters's book *From Time Immemorial* (*AHR*, 90 [1985]: 468–69)? It is not my intention to discuss Mr. Olson's review of the book. That is not my affair, and it is for Ms. Peters if she should judge it appropriate, to reply to Mr. Olson's strictures. My concern is only with the last few lines in which, after having condemned the book, he names me, along with some others, as having "endorsed, praised, and . . . apparently aided in the publication of this undeserving book." Mr. Olson has apparently not yet discovered the difference between endorsing a book and being thanked in the preface, nor has he become acquainted with the complexities of the latter. He should understand that in the course of an academic career one is often asked for help or advice by students and colleagues in one's own or other universities, as well as by other persons engaged in projects of writing and research. Such help may be of varying extent and significance; it may be acknowledged in a number of different ways, or, as sometimes happens, not at all. I can recall one occasion, many years ago, when I was young and inexperienced, a student in another university came to seek help and advice. I helped her choose and define a subject, read some of her drafts, and advised her on publication. In return, I received no mention in her preface, and was not even given a complimentary copy. A much worse case was that of a gentleman who thanked me profusely in his preface for reading his draft and making many valuable suggestions. I had indeed read his draft, and made many suggestions, which may or may not have been valuable. Since he had not followed a single one of these suggestions, I was not entirely pleased with his acknowledgment of my help. Between these two extremes of ingratitude and name dropping, there is a wide spectrum of forms of acknowledgment, in which the writer expresses appreciation for services rendered. It is not, I repeat, my intention to enter into a discussion of the merits of Ms. Peters's book. I am, however, greatly perturbed at the implication of Mr. Olson's remarks, that anyone thanked in the preface of a book may be held accountable for its opinions and

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perhaps also for its publication, even though, as in this and most other cases, he will not have seen either the book or the preface. If this is to be the accepted view, we shall have a choice between only two options; either to refuse help or advice to any but graduate students preparing theses under our direction or, alternatively, to imitate civil service procedures and require all persons consulting us to submit a final text for our approval before publication. Which would Mr. Olson recommend?

BERNARD LEWIS
Princeton University

TO THE EDITOR:

I have just seen Robert Olson's review of Joan Peters's *From Time Immemorial.* Mr. Olson claims that I have endorsed, praised, and aided in the publication of this book. This is news to me, since I have done absolutely nothing of the kind. Perhaps your reviewer will disclose the evidence on which he grounds these allegations.

ELIE KEDOURIE
London School of Economics and Political Science

PROFESSOR OLSON REPLIES:

Joan Peters's Acknowledgments leaves the impression that Kedourie did aid in the publication of the book if one considers aid in research as such. Kedourie states in his letter that he did not endorse, praise, or aid in the publication of the book. I have no reason to doubt his statement.

In the second to last line of my review, I wrote that Kedourie "*apparently* [my underlining] aided in the publication of this undeserving book." Professor Kedourie's letter is clarification of that phrase which I willingly and gladly accept.

ROBERT OLSON
University of Kentucky

Robert Olson was also invited to reply to Bernard Lewis's letter and declined to do so.

THE EDITOR

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Compiled By MELVIN HEATH and SUSAN E. G. McGOUN

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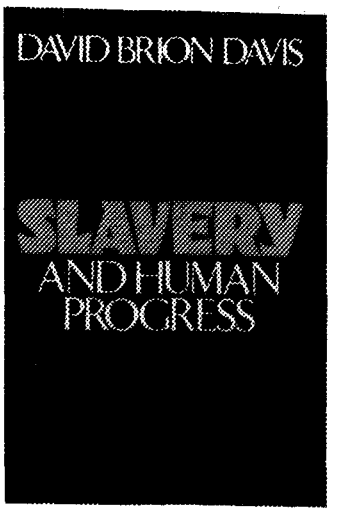
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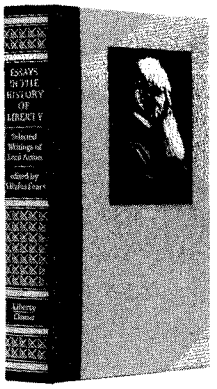
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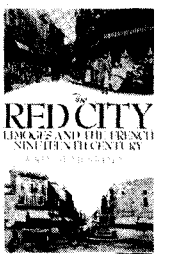
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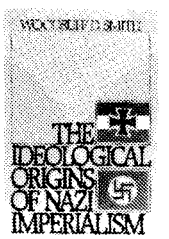
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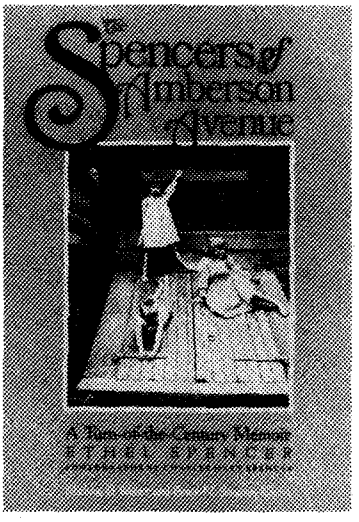
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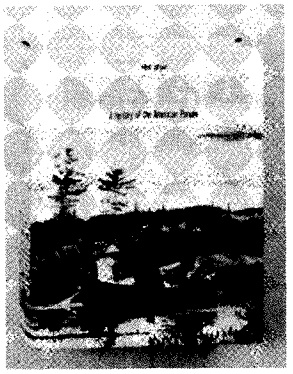
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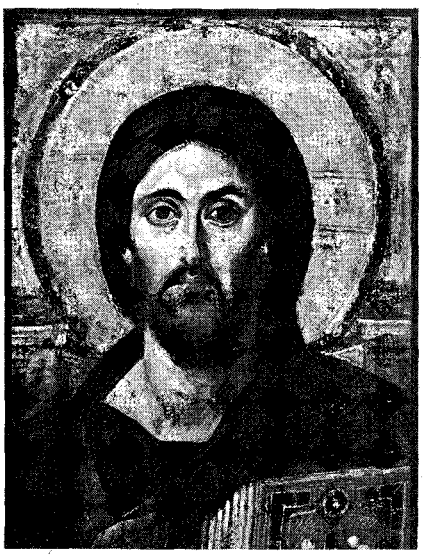
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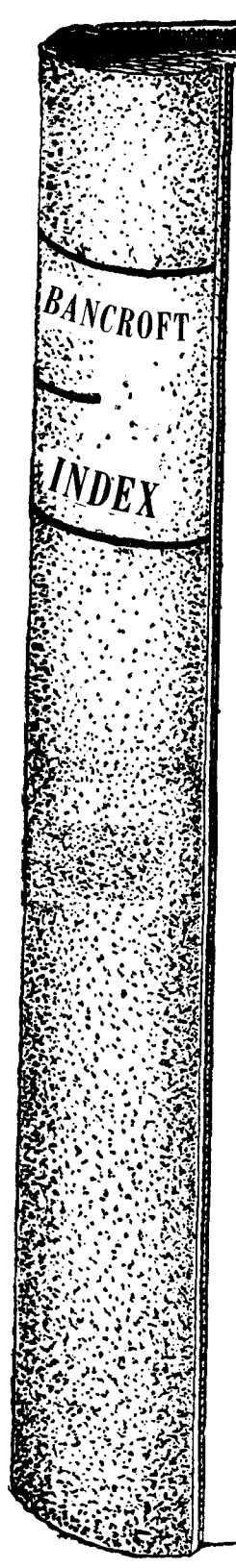
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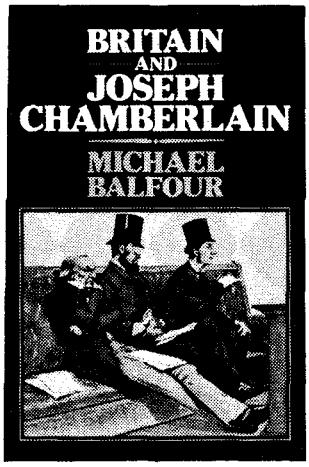
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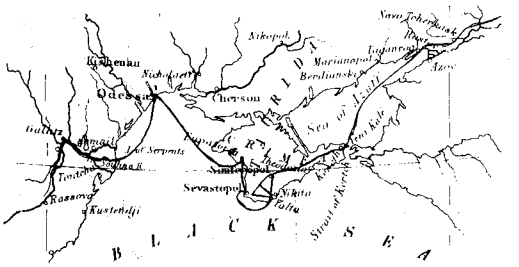
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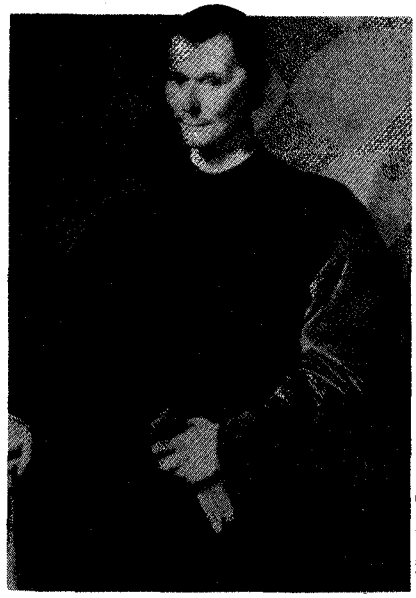
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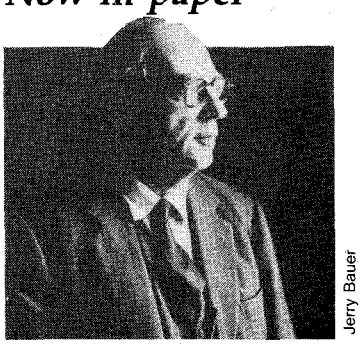
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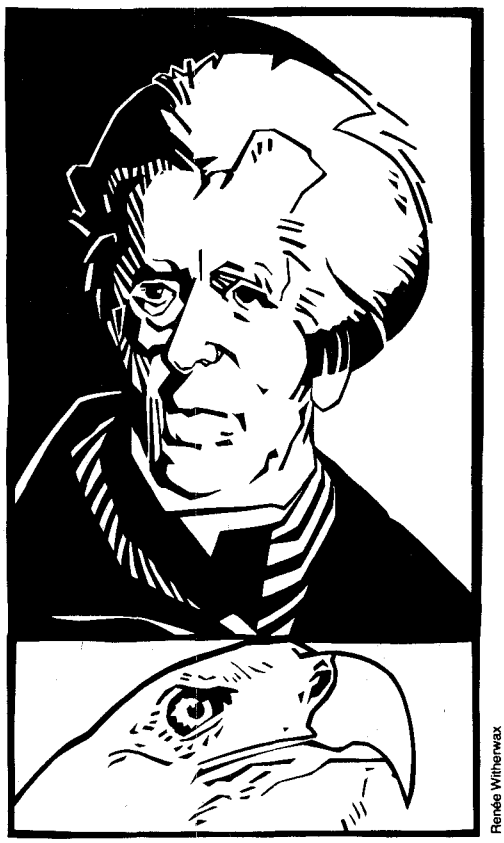
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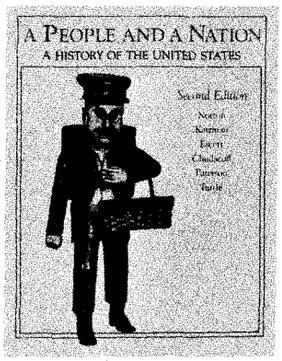
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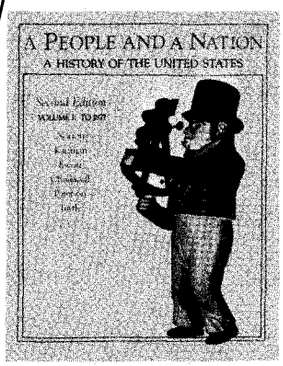
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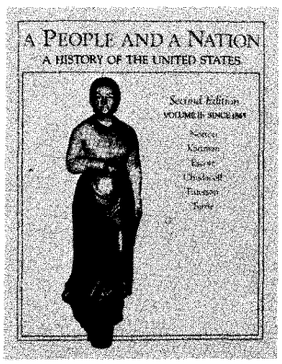
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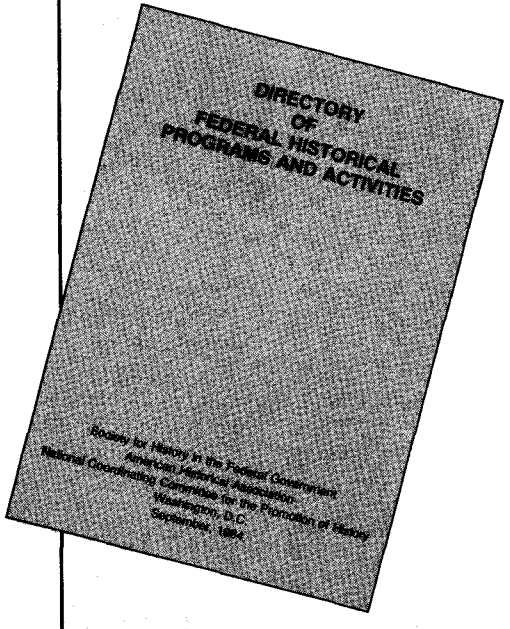
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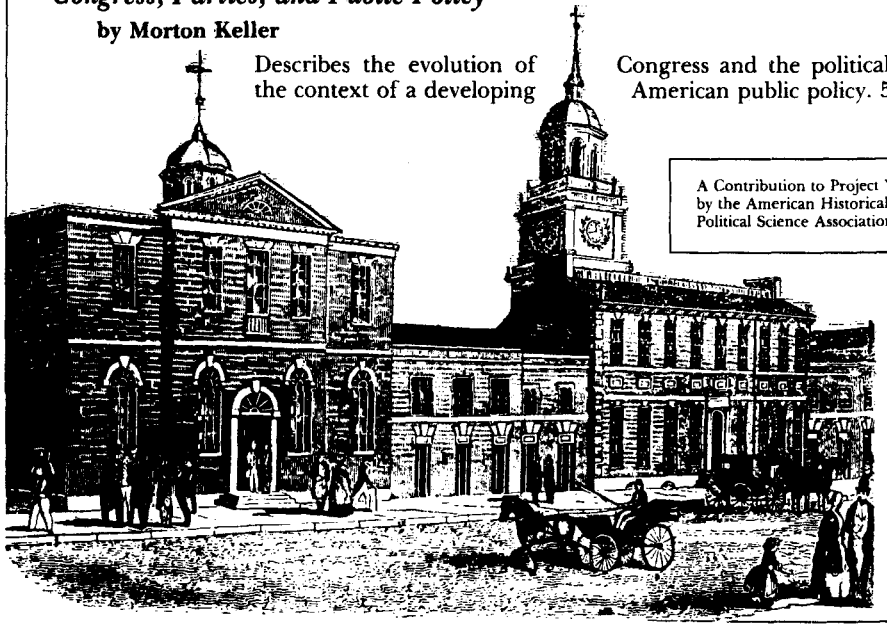
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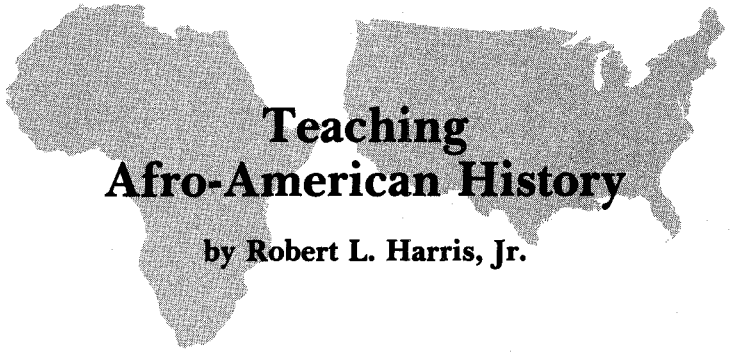
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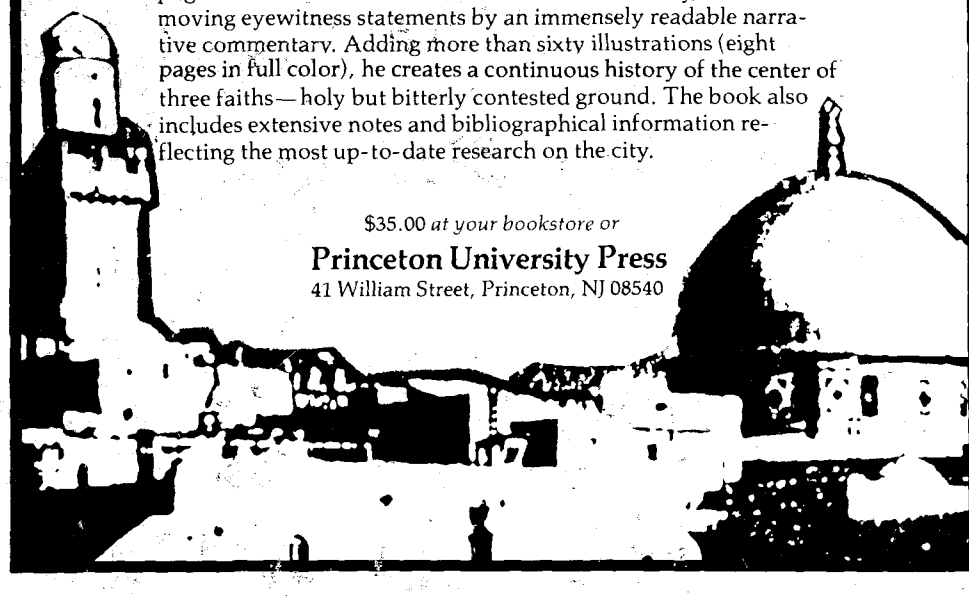
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